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Cold Fusion • New Energy • Renewable Energy

Volume 6 • Issue 35 • 2001

Cold Fusion in Japan

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Skepticism and Anomalies in Science

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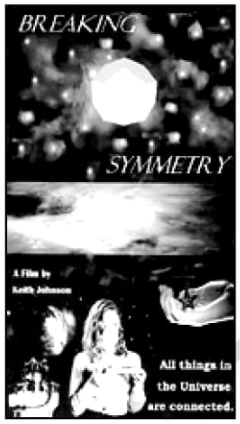

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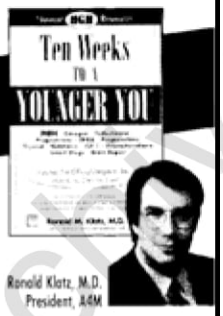

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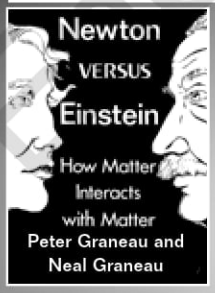

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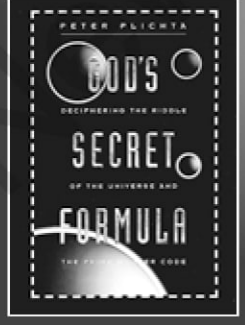

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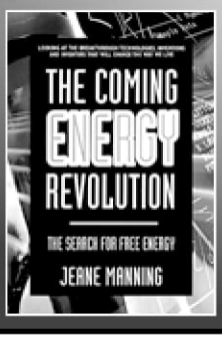

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Vol. 8, Nos. 1-2 (2000)
Accountability in Research
Policies and Quality Assurance

Cover: Photo taken on the Hokkaido University campus. The University is located in the middle of Sapporo, a major city, yet many parts of the campus are quiet and bucolic. It has the largest land area of the Japanese national universities. Photographer: Hideo Takashima. From the book *The Four Seasons at Hokkaido University*. **Cover Art:** Barbara DelloRusso.

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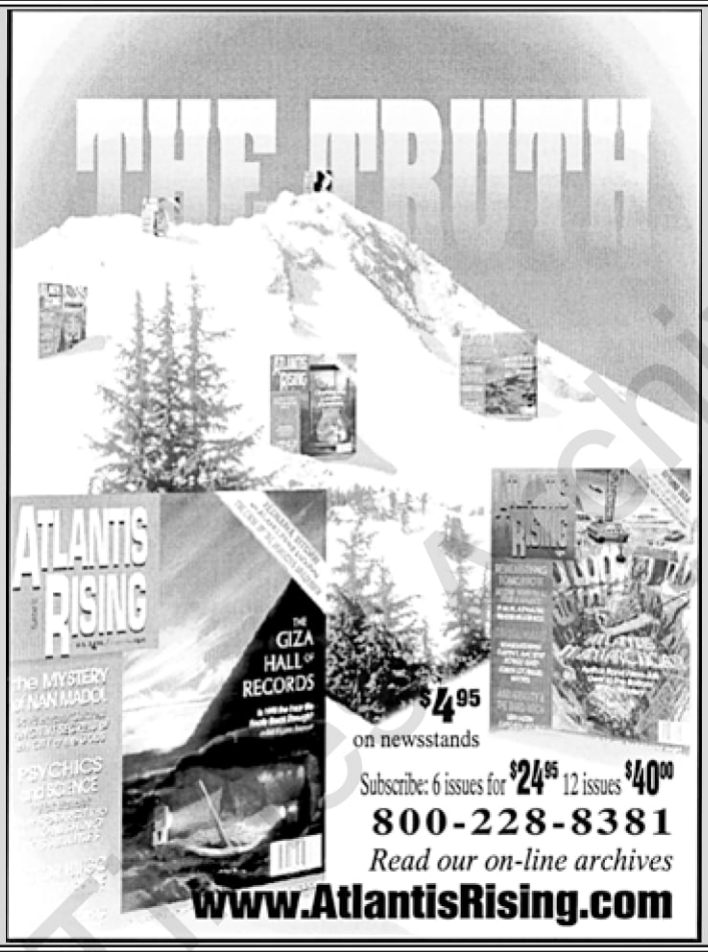

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Ethics in the Cold Fusion Controversy

by Eugene F. Mallove, Sc.D.

Long time *Infinite Energy* readers are aware that we have repeatedly and vigorously discussed ethical issues that attend the multifaceted cold fusion controversy, a battle within the scientific community that has been roiling and boiling since March 1989. Thus, we were happy to receive a copy of the academic journal *Accountability in Research* (Vol. 8, Nos. 1-2, 2000), in which eight essays assess, in the words of Editor-in-Chief Dr. Adil E. Shamoo, "The Ethical Import of the Cold Fusion Controversy." This material makes up 90% of this journal issue and is a worthwhile 162-page collection of comment and history. Unfortunately, the journal is not widely available except in specialized libraries.[1]

Dr. Shamoo, of the Department of Biological Chemistry at the University of Maryland School of Medicine, states in his lead editorial that he had formerly assumed that cold fusion had been debunked. That is, until plasma physicist Dr. Robert Terry politely told him that "the jury was still out" on the subject. Shamoo admits that in his teaching about research ethics he had often used the cold fusion episode as an example of how the "self-correcting nature of science" works. Now his view is entirely different, summarized in this key passage from his editorial:

"I find it disconcerting that competent and accomplished researchers are unable to have an open discourse about a scientific controversy in a democratic and open society. These are serious lapses from a profession (physics) that professes the highest standards of accountability. The leadership of research enterprise was lacking during the controversy. For these reasons, it is important that eleven years after the first report on cold fusion results that we discuss the process and how it impacted policy decisions. More importantly, we have to learn from this experience how to deal with these kinds of controversies in the future. Our national interest requires that we do a better job."

Shamoo was led to cold fusion theorist Dr. Scott R. Chubb, of the U.S. Naval Research Laboratory, who provided the necessary perspective and helped organize this special issue of *Accountability in Research*. Chubb introduces the series of papers by some of those who have participated in or who have reviewed the cold fusion controversy: Martin Fleischmann, "Reflections on the Sociology of Science and Social Responsibility in Science, in Relationship to Cold Fusion"; Steven E. Jones, "Chasing Anomalous Signals: The Cold Fusion Question"; David Goodstein, "Whatever Happened to Cold Fusion?"; Francesco Scaramuzzi, "Ten Years of Cold Fusion: An Eyewitness Account"; John O'M. Bockris, "Accountability and Academic Freedom: The Battle Concerning Research on Cold Fusion at Texas A&M University"; George H. Miley, "Some Personal Reflections on Scientific Ethics and the Cold Fusion 'Episode'"; David J. Nagel, "Fusion Physics and Philosophy."

Scott Chubb attributes the derailment of the proper handling of cold fusion to an early "breakdown in communication," which he writes, ". . .occurred early in the associated controversy as a result of an apparent consensus by mainstream scientists." He concludes: "Subsequently, although research in this area has continued, mainstream scientists are largely unaware of this fact. As a consequence, a large number of experiments have been carried out that are not widely known, in which positive Cold Fusion findings have been reported. Not only have these results failed to alter the predominant view that Cold Fusion is not possible, it appears that the establishment of this view, as a result of the consensus that was established early in the controversy, itself, has subverted the communication process. Thus the breakdown in communication has persisted."

Chubb is certainly correct in his assessment. I would fault him only for not identifying in more vigorous terms the driving culprits of the misinformed consensus: 1) The biased Department of Energy Cold Fusion Panel, whose unethical rush to judgement in 1989 colored all future discussion of the subject; 2) The American Physical Society's tolerance of Dr. Robert Park as its mouthpiece—a consummate dispenser of misinformation and bigotry about cold fusion; 3) The complete abrogation of minimal ethical standards within "peer review," which permits such influential publications as *Science* and *Nature* to effectively *ban* scientific papers about cold fusion from their review process; and 4) The submissive sheep-like behavior of over 99% of so-called "science journalists," who have had every opportunity to investigate and expose the establishment farce for what it is, but who have deferred to what that establishment wants from them on cold fusion: silence.

Chubb outlines what might be called an idealized basis for science: "1) scientists seek the truth; 2) because they recognize that trial and error is part of the scientific process, when scientists find flaws in what they have done, they freely admit their mistakes, attempt to correct them, and try a new approach. Thus, in an idealized situation, scientists are accountable only to themselves, and their community. If they are truthful in these endeavors, their accountability, as scientists, has been fulfilled." Clearly, this has not happened in the cold fusion controversy, as some of the essays in this very issue of *Accountability* attest.

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Accountability in Research

Policies and Quality Assurance

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Gordon and Breach Science Publishers

http://www.gbhap.com/Accountability_in_Research/

Perhaps the most egregious example is the essay by Dr. David Goodstein of Caltech, a reprint of his 1994 essay in *The American Scholar,*

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"Whatever Happened to Cold Fusion?" The essay was wrong-minded in 1994; it is even more preposterous being reprinted in 2000. (For the sake of Goodstein's reputation, its stale message should not have been included, but we can still learn from it.) Goodstein lamely admits in a five-sentence introduction: "In the years since then [1994] much has happened, but little has changed. There have been reports of increasingly reliable production of excess heat, and of the detection of ^{4}He residue, and much more. Nevertheless, the most remarkable fact remains that cold fusion has neither been accepted by mainstream science, nor has it withered away. The general situation that the [1994] article describes still seems to be in place today."

Of course nothing has changed, when a "scientist," such as Dr. Goodstein, is so disinterested, disbelieving, and uninvolved that he learns or says nothing new and dredges up an old essay in which "good cold fusion" (neutron measurements) is distinguished from "bad cold fusion" (excess heat measurements). Goodstein did not self-correct. Goodstein is not accountable—not even to himself, in the terms outlined by Chubb. Goodstein terms cold fusion a "bizarre and ugly episode in the history of science." He exults that at the Baltimore American Physical Society (APS) meeting of May 1, 1989, his Caltech colleagues Steven Koonin, Nathan Lewis, and Charles Barnes "executed between them a perfect slam-dunk that cast Cold Fusion right of the arena of mainstream science." Not mentioned by Goodstein, of course—not even in a footnote—is the published record showing that the Lewis assessment, in particular, was fatally flawed and has been so characterized and challenged in peer-reviewed publications.

Dr. Martin Fleischmann in his essay provides a comprehensive historical review of the thinking which led him and Stanley Pons to experiment with the deuterium-palladium system in the mid-1980s. He touches on social and media questions, though not heavily. And, he addresses the role that military security issues may have played in the controversy. At one point he makes a very pertinent remark: "One outcome of this research has been the demonstration that scientists have developed a blindness for accepting unusual results. No doubt this is due in part to an excessive faith in invalid paradigms." Dr. Bockris makes an equally compelling comment: "A comfortable illusion of the 20th Century—held not by scientists themselves, but by the tax payers—is that scientists are, somehow, above the fray and highly honest. What a lot of nonsense this is!"

Dr. Steven Jones in his skeletal three-page commentary confirms that he still trusts his sparse cold fusion neutron measurements—fair enough. But Jones, the egocentric denier of excess heat claims from day one, apparently has learned nothing and still knows nothing about the process of science. He is an example of the kind of scientist identified in the Bockris quote above. Jones writes disingenuously, "It is high time to strongly question claims of cold fusion based on crude techniques and to demand tests at a rigorous scientific-proof level. . .I have not seen any compelling evidence of any 'cold fusion' effects to date."

The main virtue of this special issue of *Accountability* is that discussion of cold fusion has been brought to a larger and different academic audience. Like Editor-in-Chief Shamoo, that audience may have believed the prevailing myth that cold fusion was honestly debunked in 1989. Newcomers will be able to learn from the scientific papers cited in many of the essays that this is not true. What effect this enlightenment may have in wider academe is uncertain, but it could not hurt the cause of truth.

This is not to say that this issue of *Accountability* has no shortcomings. It does. These are mainly failures of omission or insufficient emphasis:

• The absence of discussion of the questionable, *i.e.* fraudulent, "null" calorimetry experiment at the MIT Plasma Fusion Center; also no discussion of unethical press manipulation by MIT hot fusion personnel, timed for the historically critical May 1, 1989 APS meeting.

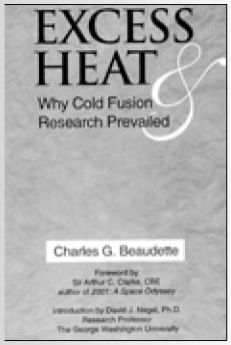
• No mention of *Nature* and *Science* magazines' particular roles as negativist actors in the cold fusion drama; no discussion by any commentator of the well-known refusal by *Nature* to publish scientific correspondence which questioned the Caltech "null" calorimetry experiments.

• No discussion of the documented pre-existing biases of those selected for the 1989 DOE Cold Fusion panel, nor any discussion of the ethical failures of some of these individuals to correct an all too evident past mistake.

• Virtually no discussion of the ethics and legality of various government agencies that hid—and continue to hide—positive excess heat results (*e.g.,* MIT Lincoln Laboratory in its secretly funded replication of the Randell Mills electrolysis experiment); no discussion of laboratory directors, *e.g.* at Los Alamos National Laboratory, ignoring internally peer-reviewed positive results in cold fusion experiments.

This is not an exhaustive list, but these are some of the issues that should fuel future discussions about ethics in science in the matter of cold fusion. To be sure, these examinations will have a much different and more compelling character when the phenomena of cold fusion eventually gain the wide support they should have had over the past almost one dozen years.

[1]*Accountability in Research* can be purchased from: Gordon and Breach Science Publishers, 1-800-545-8398, for $54. Ask for Vol. 8, Nos. 1-2, 2000.



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"Charles Beaudette has done a remarkable job in untangling and documenting the whole story of cold fusion. Excess Heat is not only a superb record of an extraordinary episode, but is also highly entertaining." —Sir Arthur C. Clarke

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LETTERS TO THE EDITOR

Hybrid Car Test Drive

I recently test drove the Toyota Prius with a "hybrid" powertrain (52/45mpg). I've waited over twenty-one years for this test drive, since I sent that letter to all the automakers in 1979 suggesting the design. The design was called the "Alpha 1."

While I cannot take credit for the introduction of the hybrid (I got the idea from an article in *Mother Earth News)*, what made the ride most satisfying is the fact that every one of the twenty or so auto manufacturers who replied advised me such an automobile could not be created.

More importantly, the hybrid experience only proves that one should not believe those who claim technological advances cannot be achieved. Specifically, when new energy is developed we'll be able to replace the gasoline engine with an on-board battery charger that needs no fuel.

The next breakthrough test drive will be in the "Alpha 1" powered by a Zeropoint™ energy converter!

John Miranda
North Bergen, New Jersey

Time of Frustration

This is a time of frustration for those of us who are working on cold fusion. The reality of Fleischmann and Pons (FP) cold fusion has been clearly established, as shown in the appended list of experiments: Techniques that always produce excess heat have been developed in Japan and transferred to the U.S., excess heat has been observed at levels far exceeding experimental error, and the helium-4 nuclear product has been measured and found to match the expectation value of 24 MeV of observed excess heat per helium-4 atom produced. Side reaction products helium-3 and tritium have been observed at levels far exceeding possible error and at quantities that cannot exist without a nuclear reaction having occurred. Also, substantial evidence has accumulated for non-Fleischmann and Pons nuclear interactions, including transmutations. Nonetheless, the scientific community refuses to look at the results, and research languishes.

In this letter I want to discuss my changed views as regards areas related to "cold fusion" transmutation physics. Helping to change my mind was the observation of scandium-48 decays by Mengoli in a deuterium cold fusion experiment. The key point involves the relationship between localized and non-localized forms of many-body physics. In particular, transmutation physics reactions require a mixed many-body/local atom form of interaction, rather than a form of many-body self-interactions that is exclusively non-local. The point is that when a delocalized (many-body) band state involving deuterons or protons shares a volume with a localized, impurity atom or nucleus, the many-body state can interact with the impurity nucleus in a nuclear fashion and extract nuclear material from the impurity atom nucleus. In the near room temperature environment the extracted nuclear matter should become wavelike. This means that it can serve as feedstock for a fusion reaction, in which no high energy particles are released. In the scandium case a deuteron extracted from an "impurity" titanium atom would have reacted with wavelike deuterium, producing wavelike helium-4 and localized scandium-48. More generally, the fusion product could be any of: helium-4, or helium-3, or tritium. With a many-body proton feedstock, wavelike deuterium could also be a product.

The above type of fusion event could be just the start of a more complex nuclear reaction process. The energized nuclear product could feed some of its nuclear reaction energy back into the daughter impurity atom to which it remains coupled, leading to fissioning of the daughter nucleus. This mode of de-exciting the product state would compete with transfer of reaction energy to the lattice, as occurs in FP cold fusion. A piecemeal transfer of energy to the fissioning atom would explain why the highly radioactive products of neutron-capture nuclear fission are avoided. The ironic point is that the very first paper Scott Chubb published on FP type deuteron fusion envisioned a reaction between wavelike deuterium and a localized product state ("impurity"). So, my guess is that most of transmutation cold fusion reactions will be found to be combined fusion-fission reactions. My prediction is that the helium fusion products will be present whenever cold fusion transmutation or fission products are observed.

The split (FP vs. transmutation) character of the cold fusion field was very apparent at the ICCF8 meeting. The unifying theme chosen for the ICCF9 meeting is coherence. Coherence is certainly one aspect of all many-body fusion processes. However, a requirement for a partitioning of the point character of the reactive hydrogen feedstock is a more specific common feature for cold fusion nuclear reactions. Coherence does apply. Coherence requires that what happens at each of the point-particle locations occurs simultaneously at all of the point-particle locations during the fusion reaction. The partitioning requirement can be viewed as an expression of Felix Bloch's periodicity symmetry principle.

These "intuitive picture" arguments can be made pretty rigorous for the FP self-interaction case, once the partitioning concept is accepted. No attempt has been made to quantify the arguments in the transmutation case, but Miley's observations of fission products that are from both endothermic and exothermic fissions essentially requires a coupling to an exothermic reaction. The fusion-fission combination seems the obvious possibility.

It will be interesting to see which of the two types of cold fusion will produce the first practical energy source for domestic heaters.

Talbot Chubb
Arlington, Virginia

List of Experiments:

Since the initial cold nuclear fusion announcement[1] there has been substantial laboratory progress in establishing the reality of cold fusion excess heat produced by radiationless d-d nuclear fusion in the deuterium-palladium system. Selected experimental achievements are listed below.

1. Increasingly clear observations of excess heat in heavy water electrolysis experiments using palladium cathodes. Early work is well-summarized in Charles Beaudette's new book,[2] *Excess Heat: Why Cold Fusion Research Prevailed.* [1989-1991]
2. Fleischmann and Pons discovery of hours-long heat production in Pd cathodes after electrolysis turn-off.[3] This phenomenon is called "heat after death." [1993]

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3. Mass spectrometer observations of helium-4 in the electrolysis off-gas in experiments by Miles,[4] B. Bush, McKubre, and Tanzella.[5] Results were presented by McKubre at the Western Regional Meeting of the American Chemical Society, Ontario, California in October 1999, and also at the ICCF8 Meeting in Lerici, Italy in May 2000. Observed helium was quantitatively measured and shown to have been produced at a rate of 1 helium atom per 24 MeV of released heat. [1993, 1999]

4. Arata and Zhang development of the DS-cathode,[6] which has produced watts of excess heat ten times in a row when used with the Arata and Zhang protocol.[7] [1994]

5. Successful transfer of the Arata and Zhang DS-cathode technology developed at Osaka University to the McKubre laboratory[5] at SRI. [1999]

6. Observation of heat and helium-4 generated in D_2-loaded 0.5% Pd-on-carbon catalyst, observed by Case[8] and verified by McKubre *et al.*[5] [1998, 1999]

7. Observation of by-product helium-3 by Arata and Zhang[9] and with great clarity by Clarke and McKubre *et al.* during study of materials from previously run DS-cathodes. Helium-3 was repeatedly observed at a helium-3/helium-4 ratio greater than 10000 times ambient value.[10] [1997, 2000]

8. Observation of tritium in gas from a post-run DS-cathode, as measured by the build-up of helium-3 in stored chemically-purified hydrogen samples by Clarke, Oliver, and McKubre *et al.*[10] [2000]

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AquaFuel-type Patent

In *IE* No. 9 (p. 44), you say, "Why didn't anyone notice this before?" about AquaFuel. My patent is simpler: I use an acid to produce free radicals from grass, sugar, or other hydrocarbons. Each free radical dissociates more than 10,000 water atoms to produce O and H_2. This is more than over-unity.

O.M. Baycura
Campbell, California

Editor's Note: Orestes M. Baycura holds U.S. Patent #5,100,642, "Method of Generating a Fuel from Water and a Compound Containing Free Radicals." Abstract:

> A fuel composed of water, a compound in which free radicals have been generated and a solute that stabilizes the free radicals while the fuel is in the liquid state but which releases the free radicals when the fuel is separated (vaporized) so that the free radicals then dissociate the water to form hydrogen gas in sufficient concentration to support combustion. The most readily available compounds in which free radicals can be generated to provide the fuel are hydrocarbons and carbohydrates. Free radicals may be generated in solids (*e.g.*, sugar) by charring and grinding the sugar then mixing with acid. Free radicals may be generated in liquids by adding an acid. Polar liquids such as alcohols, aldehydes, and ketones have been found to be the most effective stabilizers.

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Fogged Film Ignored

Back in 1989, a researcher at my alma mater *(Editor's Note:* University name withheld by request.) was trying to duplicate the Pons-Fleischmann experiment. She left the loaded Pd cathode on some film in her desk drawer, and *a la* Mdm. Curie, later discovered the film fogged. Her director would not let her progress further for fear of embarrassment if it were not true. Lots of schools were getting bad press at that time regarding cold fusion experiments. I think she quit in protest and went to another school.

Wil Wakely
San Diego, California

Editor's Reply: This is a wonderful anecdote, since many cold fusion researchers have found such fogging. It is too bad that the normal curiosity of scientists was not allowed the free reign it deserved. —EFM

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Truth in Media?

I am writing in response to Dr. Mallove's editorial in Issue 34. You obviously feel cheated and shunned by mainstream media due to the fact that cold fusion has not been promoted in a positive light. As a veteran television producer, I share your frustration. Over the past fifteen years I have witnessed story after story reported in a biased or "sugarcoated"

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manner. The majority of the time the most important stories are simply never told due to the political policies of the controlling interest.

The election controversy we are currently witnessing is one of many such examples. I have personally investigated numerous reports of voter fraud while covering this issue. Many other news journalists, reporters, producers, and cameramen also had the opportunity to report on these injustices but failed to do so. Why? Most of the larger organizations are governed by the politics of the almighty advertising dollar or the vested interest of the conglomerate puppet masters.

In this delusionary world of greed, ego, prejudice, and fear that we live in, truth is sometimes only found in the heart. All of those who battle against skepticism in search for a better world should consider the significance of the seeds that they have planted for future generations. We may not be able to change the world today but we do make a difference for tomorrow.

Keep up the good work; we are counting on you.

Douglas Rodante
Palm Beach Gardens, Florida

Errata

Photos of the Gulko/Beller device on the cover and photographs within the article of *IE* No. 34 were taken by professional photographer James Downer of New Vision Photography (e-mail: newvision@erols.com).

The address for the Alternative Energy Institute (AEI) mentioned in Dohn Riley's "The Coming Energy Crisis" in *IE* No. 34 is P.O. Box 7074, Tahoe City, CA 96145, U.S.A. • Phone 530-583-1720 • Fax 530-583-5153 • http://www.altenergy.org.

The AEI is a 501(c)(3) non-profit educational organization deeply committed to raising public awareness and generating a healthy concern regarding the very real limitations and consequences of burning fossil fuels, as well as a positive attitude toward new energy research and utilization.

The date for final submission for the Energy Writing Contest has been changed to May 1, 2001 (see page 31). In Issue 34, the date was improperly noted.

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Soo Seddon, our U.K. correspondent, contacted Dr. James Lovelock to see if he would be willing to submit a paper to *Infinite Energy*. His letter in response follows. Lovelock was born in 1919. In 1957 he invented the electron capture detector, the device which proved paramount in discovering that pesticide residues were polluting natural resources and that nitrous oxide and CFCs were becoming concentrated in the atmosphere. In the 1970s Lovelock first put forward his Gaia theory which proposes that the Earth is a single living system in which interrelated mechanisms regulate and maintain optimal conditions for life. In effect, that life itself creates and maintains the perfect conditions under which life can thrive. Lovelock's theories and inventions have greatly contributed to mankind's understanding of the environment. Gaia, although still not a totally accepted scientific theory, has a potential value that cannot be underestimated. Soo will be reviewing Lovelock's latest book in the next issue of *IE*.

Thank you so much for your thoughtful letter and for sending a copy of the latest issue of *Infinite Energy*.

First, I stopped subscribing to your journal because in 1998 I ceased serious science and passed my research on to my successors. This action brought a drop in income and a need to rationalize journal subscriptions. I now only take *Nature, Science, New Scientist,* and *Scientific American*. I would have liked to continue with *Infinite Energy* because it is an interesting and intruiging journal and a pleasure to read.

You may be unaware that I have always doubted the Pons-Fleischmann research, not from prejudice but because in the 1960s I was much involved with palladium hydrogen systems for JPL in connection with the Viking mission. Pulses of heat happened fairly frequently and were traced to the episodic and non-linear oxidation of hydrogen stored in the palladium cathodes. If only cold fusion in the palladium system happened it would have been wonderful, but it seems unlikely that it ever did.

It is good of you to offer the chance of an article expressing Gaian thoughts on future energy supplies and needs, but Gaia theory is still a long way from acceptance and until it is such an article would not have much force.

I am grateful for your interest and wish *Infinite Energy* every success.

James Lovelock
Launceston, England

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"Websites Worth Your While"

http://www.fueleconomy.gov

At this site, you can compare your car's gas mileage to other models and learn how cars process gasoline, among other things.

http://tryscience.org

Looking for a neat science experiment to perform with the kids? This site contains hundreds of experiments well-suited for children, some of which can be done online while others are perfect rainy day afternoon fun.

http://www.daystarcom.org

This site tackles life's big questions, such as "Is there now scientific evidence for God?" The objective of Day Star Network is "exploring and debating the ultimate questions raised by science." Responsibly executed, the site features articles, interviews, and debates by noted astronomers, biologists, and physicists, who have perspectives ranging from atheistic to conservative religious opinion. It features a discussion/comments forum as well. Erudite editor Fred Heeren also publishes a quarterly magazine, *Cosmic Pursuits*, plus books and tapes which have the same themes.

Infinite Energy does not endorse these sites or products.

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Report on the Second Annual Japan Cold Fusion Society Conference (JCF-2)

by Jed Rothwell

The Japan Cold Fusion Society (JCF) is an informal organization with forty-four members, who are mostly professors at national universities. (See http://fomcane.nucl.eng.osaka-u.ac.jp/jcf/index.html.) Thirty-five or so attended the Second Annual JCF Conference (JCF-2), October 21-22, 2000, on the campus of Hokkaido National University, in Sapporo. It was a low-key affair. The abstracts and viewgraphs were in English, but most of the presentations were in Japanese. Three non-Japanese attended: F. Celani, A. Arapi, and me. Twenty-seven abstracts were submitted. Two authors did not show up, so twenty-five presentations were made, in eight categories and eight sessions, including two theory sessions. A few papers were disappointing, especially in the light water electrolysis session on the first day. Five or six presentations were outstanding, particularly those of Iwamura *et al.* (Advanced Technology Center, Mitsubishi Heavy Industries), and Mizuno *et al.* (Hokkaido and Osaka National Universities). The Takahashi group at Osaka National University continues to report progress with ion beam implantation.

This report covers my impressions of the experimental papers. The theory papers are over my head. Readers interested in theory are encouraged to look at the web page, which includes both abstracts and the "OHP" (overhead projector slides; viewgraphs) from several researchers.

Iwamura *et al.*, the Conference Highlight

Most participants agreed that Y. Iwamura, M. Sakano, and T. Itoh were the stars of this conference. Iwamura and his colleagues have been running cold fusion experiments since 1993. They reported positive results at three ICCF conferences (see one of their reports in *IE* No. 20, p. 56). They use what I call a "membrane" palladium cathode. This is a thin, square plate mounted at the bottom of a cell, which is filled with heavy water electrolyte. Below the cathode, the bottom of the cell is open; it has holes leading to a second chamber, beneath. This second chamber is evacuated, so that in effect the cathode forms the bottom of the cell wall. It is bolted to the bottom of the cell, with an O-ring gasket to prevent the water in the cell from being sucked around the edges of the cathode into the vacuum chamber. (See Figure 1.) The anode is a plate mounted above the cathode. The deuterons load into the top surface of the cathode, then migrate through it and out of the bottom into the vacuum chamber.

When Iwamura began these experiments, he used pure palladium cathodes, and his success rate was low. Later, he devised a working model (a theory) which he calls the "electron induced nuclear reaction model," and based on it he concluded that cold fusion requires three conditions:

1. A high D/Pd ratio "on the palladium surface." Most cold fusion working theories emphasize high loading in the bulk of the metal.

2. Rapid diffusion flux of deuterium through the palladium. McKubre also emphasizes this. It is a function of the equation he developed to predict the level of excess heat.

3. The existence of a second material in the palladium, which has a high work function (a high amount of energy is required to remove an electron from the metal surface). He has tested CaO, TiC, Y_2O_3, and other alloys. A layer of this material is sandwiched between two layers of palladium. (See Figure 2.)

Whether the model is ultimately proved valid or not, the technique of adding the second material was remarkably effective. Reproducibility increased dramatically. For the past three years, Iwamura and his colleagues have run the experiment two or three times a year, and it has produced excess heat and other nuclear effects every time. This is a completely repeatable cold fusion experiment. It is "repeatable" now, and it will be "reproducible" after another laboratory performs it successfully.

Electrochemists at the meeting thought this technique works for prosaic reasons having nothing to do with nuclear theory. With a normal configuration, both sides of the cathode are loaded. Heavy water electrolyte always contains some light water (hydrogen) contamination, which hampers the cold fusion effect with palladium. Both deuterium and hydrogen are forced into the cathode. They gradually leak out, but the cathode preferentially retains hydrogen, so the concentration of hydrogen in the cathode is more pronounced than in the surrounding water. In the Iwamura cathode, deuterium and hydrogen enter one side of the cath-

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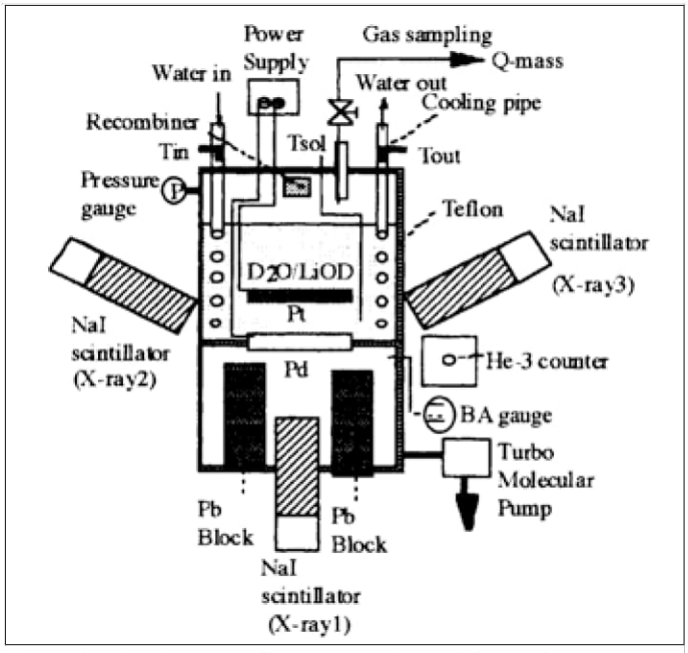

Figure 1. Continuous Diffusion Experimental Apparatus.

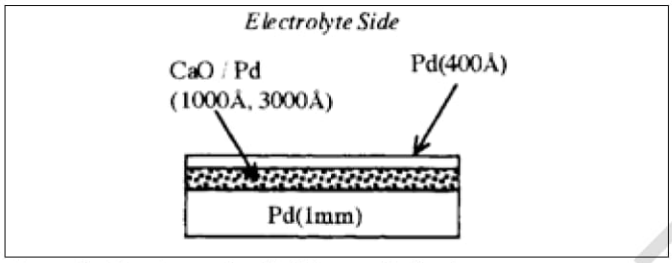

Figure 2. Structure of a Multi-Layer Cathode.

ode, travel through it, and come out the other side in a steady stream. The ratio of deuterium to hydrogen remains the same because the moving deuterium gas purges hydrogen and other gas contamination. Not all experts agree; Ed Storms expects the ratio will be the same in the Iwamura cathode as it would be with a regular Pons-Fleischmann cathode.

The mass of transmuted products found in Iwamura's cathodes is proportional to the excess energy; more energy makes more products. Control cells with hydrogen (ordinary water) do not produce heat or apparent transmutations. This is not to say the products are "commensurate" with a particular nuclear theory, the way the helium in McKubre's cell is commensurate with conventional D-D fusion. It would be difficult to measure the mass of all transmuted products in Iwamura's cell with enough precision to confirm a nuclear theory.

Contamination is the critical issue in this experiment. Electrolysis gathers up all the metal contamination in the electrolyte and deposits it on the cathode surface. Iwamura takes many precautions to exclude copper and other contamination, but if tiny amounts did slip in, they would end up on the cathode surface. How does he know the copper really is transmuted from palladium, and not contamination? Two ways: first, he takes a complete inventory of the contaminants in the cell. He finds, for example, there is more silicon on the cathode surface than there was initially in the electrolyte and cell components combined. (The cell itself is made of Teflon, not Pyrex or other glass made from silicon.) It would be impossible for all silicon atoms in the wires and cell walls to leach out into the electrolyte and on to the cathode surface, but even if this could happen it would still not explain the results. Second, he measures the isotopes of the transmuted material. Samples of iron collected anywhere on earth, from asteroids, or brought back from the moon all have the same isotope ratio. It is always 92% Fe-56, 6% Fe-54, 2% Fe-57, and less than 1% Fe-58. The only exception is iron which has been left in a nuclear reactor, or iron which was transmuted from some other element in a nuclear reaction, which is "unnatural" or "isotope-shifted." Many of the elements found on the cathode surface have isotope shifts. The shifts vary with elements, and they are not consistent from one run to another. A small difference in the isotope ratios may be due to the error margin of the mass spectrometer. Iwamura measures the ratio of Fe-57 divided by Fe-56, which in nature is 2/92 = 0.02. In three readings from Iwamura's spectrometer, natural iron came out at 0.023, 0.023, and 0.022.

Some of the elements found on the cathode surface have isotopes with natural ratios, which leaves open the possibility they may be contamination. Others, particularly iron and silicon, have isotopic ratios far different from those occurring in nature, and far greater than the error limits of the instruments. At ICCF-7, Iwamura reported the Fe-57/ Fe-56 ratios in six samples. In one sample the ratio was measured three times at 0.035, 0.038, and 0.065. The difference between 0.022 and 0.035 is beyond the uncertainty of the mass spectrometer. In other experiments, the ratio changed dramatically: to 0.26, 0.29, and 0.66, an order of magnitude higher than the natural ratios. In common with results reported by Bockris and Miley, the isotope shifts vary with depth. Close to the surface of the cathode, the isotope shifts are dramatic. As you strip away layers of the cathode metal and go deeper into the material, the ratios return to natural. This could be because there is only a little copper at the deeper levels, and it is mainly contamination. Or it could be caused by the nature of the nuclear reactions, which presumably occur at or near the surface.

The iron ratio reported at ICCF-7 was measured with SIMS spectroscopy. It has been measured again recently with Time of Flight-SIMS (TOF-SIMS). Iwamura *et al.* measured the isotopic ratios of Si with another method: ICPMS (Inductively Coupled Plasma Mass Spectrometry), and they found the amount of Si-29 was four times lower than it is in natural samples. (Results from spectroscopy are usually presented as ratios, but sometimes they are normalized.)

Sakano described a new set of experiments that expand the Mitsubishi research program, and confirm the original work. In the new experiments they use similar detection equipment and multilayer membrane palladium plates, as before, but they employ gas loading instead of electrolysis. In the original experiment, which is still continuing, they use liquid elec-

The Iwamura research program is a masterpiece. This is the most dramatic and clear-cut proof yet revealed that cold fusion transmutes metal, and it is one of the most important experiments in the history of the field.

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trolysis, and they observe excess heat and massive transmutations in every run, with occasional bursts of X-rays and gamma rays. Spectroscopy is performed on the cathode material before and after the run. In the new experiment, the top chamber is loaded with low-pressure (1 atm) deuterium gas. (Higher pressure might load the metal faster, but it might rupture the thin membrane.) The deuterium is absorbed into the top surface of the palladium membrane, and it is gradually sucked through the metal into the vacuum chamber below. The palladium undergoes transmutations similar to those observed with electrolysis. Gas loading pushes the deuterons in more slowly than electrolysis, so the reaction rate is thousands of times slower. They assume it would be impossible to detect the excess heat at such a low rate, and they do not perform calorimetry. The reaction is slower, but contamination is also reduced by several orders of magnitude.

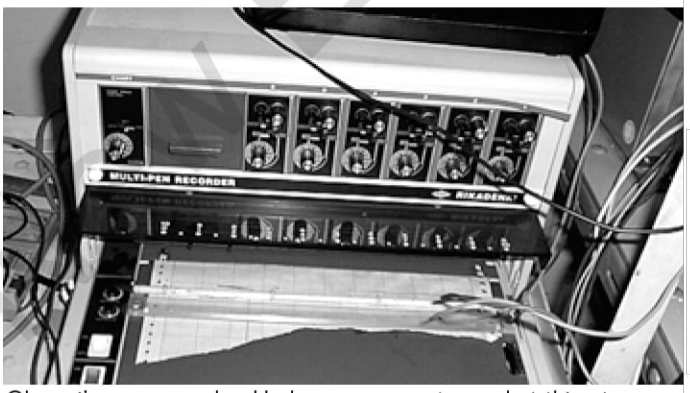
Another major advantage to the gas loading experiment is that it allows them to watch the transmutations as they occur, in the cell, without removing or disturbing the palladium sample, which might introduce contamination. An on-line, *in situ* X-ray mass spectroscope is installed in the top chamber. At the beginning of the run they evacuate the top chamber, and they examine the surface of the palladium and inventory the elements and isotopes in it. As the experiment proceeds, every week they evacuate the deuterium gas again, and take another reading with the mass spectroscope. They see surface transmutations gradually develop and change. The evolution of these transmutations is complex and astounding. As far as I know, no theorist predicted it. Magnesium, for example, appears early in the run, increases, and then it begins to decrease. Apparently when the magnesium atoms reach a certain critical density, the process changes and they begin to transmute into some other element.

After the run, various methods of off-line spectroscopy confirm new element production and isotope shifts. The total mass of some of the new elements is much larger than all sources of contamination can account for. In a few cases, it is so large that the products are visible, forming a small pile of material about the size of the graphite in a pencil point, as shown in a photograph. These macroscopic transmutation products are mainly elements lighter than iron, silicon and sulphur, which means their nuclear synthesis is endothermic. Apparently, exothermic nuclear reactions (ones that produce net energy) trigger endothermic reactions. A purely exothermic nuclear reaction that produces visible amounts of ash would generate fantastic amounts of energy, so other reactions in the cathode must be absorbing some fraction of this energy. Otherwise Iwamura *et al.* would be vaporized in a nuclear explosion.

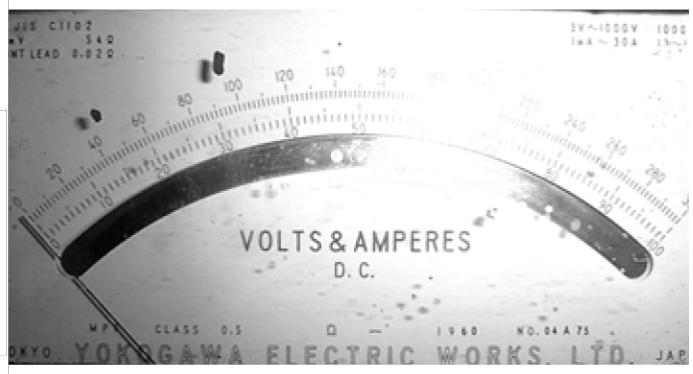
The Iwamura research program is a masterpiece. This is the most dramatic and clear-cut proof yet revealed that cold fusion transmutes metal, and it is one of the most important experiments in the history of the field. The quality of the data, the care, precision, and high signal to noise ratio is rivaled only by the results obtained by McKubre. These are also the most expensive experiments in the history of this field. The dedicated, *in situ* spectrometer alone costs an estimated $700,000, and it is bigger than Mizuno's entire laboratory. The entire experiment is performed in a clean room, which must cost millions. The NHE demonstrated that millions of dollars do not guarantee good results. Ohmori does far better experiments than the NHE did, using a few thousand dollars worth of equipment, including mercury thermometers, analog meters manufactured in 1960, and a wind-up mechanical stopwatch. Mitsubishi has demonstrated that in the right hands, massive funding and the finest equipment money can buy will prove that cold fusion is real, and will go far to elucidate the nature of the reaction.

McKubre proved once and for all that palladium cold fusion produces helium. Iwamura has proved it also produces transmutation, and fission. I have no idea how to reconcile these two findings. The ratio of McKubre's heat and helium production is within 5% of the conventional hot fusion D-D reaction (going to He-4), which is nothing like heavy metal fission. Fission also produces helium (alpha particles), but can it mimic D-D fusion rates? I asked a group of physicists in Washington, D.C. whether a cold fission reaction will produce enough helium. They chorused, "Sure, yeah, lots more helium in some cases—maybe more than fusion." I responded, "Okay, can it produce precisely as much helium as D-D fusion, within a few percent, even when the reaction products vary?" Silence.

Ed Storms has a hypothesis. He assumes McKubre must be producing these transmutation reactions in the cathode, since they seem to be so common. McKubre has concluded the principal reaction is definitely fusion; a D-D reaction resulting in helium and occasionally tritium. He hedged when I asked him whether he has observed transmutations in the cathode. He said that years ago they checked palladium cathodes for signs of transmutations. They found confusing, unconvincing evidence of foreign material with possible isotope shifts. McKubre is now using a Case cell with palladium deposited on industrial carbon catalyst. It would



Ohmori's pen recorder. He has no computer and at this stage no electronics for the cell at all so even the pen recorder cannot be used. All data collection is manual with alcohol thermometer and a stopwatch—a mechanical wound stopwatch at that.



Ohmori's ammeter, manufactured in 1960, Yokogawa Electric Works, Ltd.

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be impossible to detect transmutations in this material. McKubre's ratio of heat to helium must include alpha emission from transmutation, and this might partly explain why the amount of helium is somewhat lower than the expected value for D+D fusion. The amount of helium contributed by transmutations is probably much smaller than the D+D fusion contribution. Therefore, Iwamura must be getting most of his excess power from fusion. Hydrogen fusion produces much more energy per nucleon than heavy element fission. In other words, Storms postulates that the fission transmutation reactions always accompany fusion.

I asked Ohmori whether the element production and isotope shifts reported by Iwamura and in glow discharge experiments at Iwate University are in line with results reported by Ohmori, Mizuno, Bockris, and Miley. He said these results are in reasonable agreement, especially for iron and copper, and the differences can be explained by several factors such as: different cathode materials; varying methods of spectroscopy which look at the surface or the entire bulk; and the fact that many reactions in the Iwamura layered cathode probably occur at the boundaries between layers, which cannot be detected with the *in situ* spectroscopy, and which Iwamura has not yet investigated in detail.

I asked Iwamura whether these experiments have been independently replicated or otherwise confirmed. They have not been replicated, because of the expertise required and the astronomical cost. However, the results have been independently confirmed. Used palladium cathodes have been "farmed out" to laboratories in Japan and Europe for specialized spectroscopy and other tests. These independent tests with different instrument types confirm the isotope shifts, which proves the new elements cannot be contamination. The shifts are orders of magnitude greater than changes induced by things like SIMS or by electrochemical isotope separation, which in any case cannot occur in the gas loading experiments.

Other Interesting Papers

Yamada *et al.* at Iwate University have performed welcome replications of other people's research, in two sets of experiments with nickel and gold. Using three simple isoperibolic calorimeters, they did nickel light water experiments with platinum anodes. They found that untreated nickel does not produce excess heat, but *scraped* nickel produces up to 10% excess in nearly every case. This is not much in percent terms, but they have confidence in the calorimetry. I am concerned about the data because the excess heat points seem to be lined up a little too neatly, equidistant above the calibration curve, the way they line up when caused by a predictable chemical process like recombination, or an instrument error. Cold fusion excess heat is usually less predictable, with data points scattered at random above the calibration curve. Excess heat is not necessarily a smooth linear function of applied current, especially not with nickel light water systems. (See Yamada's graphs at the JCF website, OHP for JCF2-3.)

Yamada's group did another set of studies of transmutations in gold cathodes with light water, replicating Ohmori. Gold is a particularly good choice of material for this purpose because it has only one isotope, and very pure samples are available. Highly accurate TOF-SIMS spectroscopy was successfully used to identify transmutations and elements with anomalous isotopes in the gold cathodes. There was a marked increase in amounts for mass numbers from 64, 66, 67, 68, and 70. The most notable increase was for mass number 67, which probably represents Zn-67, in concentration of 15% compared to the natural distribution of 4%. Increased counts for nickel were also observed, but the isotopic distribution was natural. EPMA analysis of the gold cathodes revealed zinc as well as nickel. Ohmori measured excess heat with gold cathode transmutation. Yamada did not look for excess heat; he did not measure the cell temperature. This may be a wise decision, because temperature sensors can introduce contamination.

Another group of researchers at Iwate University, A. Arapi *et al.,* described experiments using autoradiographs, the tried-and-true late nineteenth century method of detecting radioactivity. Radiation was detected in several instances. Researchers say it is surprisingly difficult to detect such low-level radiation with more modern techniques and fancy instruments. The cathodes were plates, and they were placed in a holder which kept them vertical, with film placed on either side of the cathode, separated slightly from the metal surface to prevent the remote possibility that evolved hydrogen, deuterium, or recombined water might contaminate the film. The skeptics have suggested that hydrogen or water contamination might cause the film to develop black spots in the absence of radiation. As far as anyone knows, hydrogen does not register on X-ray film, which is sealed and protected. If this could happen, it would be a major discovery about photographic film chemistry, which should make the skeptics anxious to confirm their suspicion! In response to the hydrogen contamination hypothesis, researchers at BARC in Bombay, India produced autoradiographs with electronic detectors designed to replace X-ray film.

Another welcome replication is underway at Osaka University, where M. Ohta and M. Matsunaga at Osaka University are doing glow discharge experiments first reported by Ohmori and Mizuno. They observed the production of Fe, Zn, and Ca. Mizuno thinks they will see more dramatic results if they increase voltage.

H. Numata described what happens when different electrolysis methods are used to repeatedly load and deload palladium. It is known that loading and deloading will "alter the microstructure" of palladium, making it distort and expand permanently. This research looks very capable. The results are in line with those reported by Storms and others, and they go well beyond it because Numata measures *in situ,* with computer controlled instruments measuring electromotive force, resistance, and dilation (expansion). This is more flexible and easier than Storms' method of removing the cathode from the cell to measure expansion. Numata makes sophisticated observations with an array of instruments, so he can detect in the same sample the coexistence of alpha and beta-phase loading, and voids—gaps in the metal lattice which prevent the cold fusion reaction. Void formation apparently occurs during the transition from alpha to beta-phases.

Numata does not appear to be familiar with the work of earlier researchers such as Storms. He may be reinventing the wheel in some ways. He may not realize the significance of his own results; for example, he does not appreciate that other researchers have correlated expansion and excess heat performance, and have shown that when palladium expands more than a small percent because of void formation, it does not produce excess heat.

Mizuno and Ohmori described recent results in glow dis-

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charge plasma electrolysis, described below.

In the first half of his talk, F. Celani described excess heat and nuclear effects with thin wire. The second half was devoted to two new species of bacteria they have discovered in heavy water (genere Ralstonia and Strenotrophomonas). DNA tests confirm that these are new species, and they are now listed in national gene banks in the U.S. and Japan. They are ubiquitous, being found in samples of heavy water from different suppliers. They are remarkably difficult to kill. They survive 80˚C heating, and 30 mega-Rad radiation from Co-60. This is an important issue with some cold fusion experiments, because microphotos of palladium wires show that the surface is covered with these bacteria after a few days, which blocks loading and changes the surface chemistry. With conventional electrochemical cold fusion, the concentration of lithium salt is usually high enough to kill the bacteria, but in Celani's cells LiOD is less than 0.1 molar, and the bacteria seems to thrive on the mercury compounds Celani adds as a surface treatment of the wire. Other chemicals, strong enough to kill the bacteria, also poison the cold fusion reaction.

Accelerator Beam Loading

Several papers were presented describing the practice and theory of the accelerator beam loading experiments at Osaka University. This research looks carefully done and the presentations were professional, but I do not understand it well enough to comment in detail. Interested readers should review the abstracts and viewgraphs on the website.

For many years, A. Takahashi and his colleagues have performed a series of experiments at the ion beam facility at Osaka University. The beam is an accelerator 30 meters long, costing millions of dollars, which fills a warehouse-like facility. Takahashi's group does not have exclusive use of it, but they do have a share of the monthly experiment time, which they devote to cold fusion research. They use other, smaller machines as well. A metal target, usually titanium, is mounted in a vacuum and the beam injects deuterons into it. With electrochemistry or gas loading, the difficulty is to saturate the metal with deuterium over a long period. With a beam, the hard part is to keep the sample from vaporizing in the intense stream of particles. (The sample is mounted on a plate which is cooled by liquid nitrogen to prevent this.) The moment the metal is loaded it begins reacting. It produces gamma rays, charged particles, and other easily detected radiation. Many of these reactions are conventional, well understood fusion reactions caused by deuterons piling up on top of one another in the metal, but Takahashi believes that others are caused by "multibody cold fusion." The difficult part is to separate the conventional reactions from the cold fusion reactions. This is done by using different kinds of detectors, filters of different thickness, coincidence measurement, and various other methods. ("Coincidence" means two detectors work simultaneously, to eliminate local background and false events, and it also refers to two detectors separated by a fixed distance to measure Time of Flight, TOF.)

At this conference, Osaka University researchers reported on experiments implanting deuterons and protons into titanium, and electrons with another machine. The electron beam did not produce any anomalous effects, which was more or less what they expected.

Takahashi has a theory which he calls the "multiphoton excitation" model, which he says explains both the ion beam results and the distribution of transmutation products in electrochemical and gas loaded cold fusion.

Some Less Impressive Papers

I went to the conference with high hopes for positive results from a nickel light water experiment at Yokohama National University. Unfortunately, this project seems stalled, and poorly thought out. At ICCF-7 K. Ota said he expected to see positive results soon, and they had just finished several months of improving the calorimeters they inherited from the NHE project. At JCF-2 he described the experiment in detail, and reported no significant excess heat. It turns out, they spent months improving the heating flow calorimeter heat recovery rate from ~96% to 99.01 +/- 0.34%. This effort made no sense to me, nor to several members of the audience I spoke with. The recovery rate was quite stable and predictable at 96%, and the fact that it was 96% can be established by first principles of physics, so you could always extrapolate the remaining 4% with confidence. The improvement does nothing to improve confidence in the results. They have put a great deal of effort into making the calorimeter temperature extremely stable. This improves precision, but unfortunately, as Fleischmann and McKubre have often pointed out, it imposes a stable, unchanging temperature on the cell, which probably prevents the cold fusion reaction from occurring. They have improved the instrument too much, and in the meantime they did not spend enough time thinking about the cell and cathode. It is intended to be a replication of the Patterson CETI cell, but instead of using thin film beads, they use nickel rods chopped into 1 mm segments. These short cylinders are shaped somewhat like beads, giving them more surface area than rods, but they do not load quickly the way thin film does.

R. Notoya described gamma ray observations during light water platinum electrolysis. She sees several energy peaks which she ascribes to various isotopes of potassium, titanium, calcium, and scandium. Other researchers find it difficult to accept these results, because the background noise from natural sources is so high, and because many other people made intense efforts to detect gamma rays from early in the history of the field, but most of them failed to detect anything. Others, like Iwamura, see only sporadic gamma emissions.

Several lesser-known researchers and private individuals showed up at the conference, and some of them gave papers on theory which did not impress the other participants.

T. Hanawa, Osaka U. Emeritus, gave an interesting presentation about the effect of water in "biological nuclear transmutations," which are apparent nuclear reactions in plants and animals. These were studied by Kervran and others, long before cold fusion was first reported. Hanawa's presentation was beyond the scope of the conference, and far beyond the scope of my knowledge, but it seems a shame that this area has been neglected and marginalized by the mainstream. Mainstream scientists would probably find transmutations in bacteria and seeds even more unbelievable than transmutations in metal lattices (see review of *Biological Transmutations* in *IE* No. 34).

JCF-2 Publications, Web Page

JCF president A. Takahashi asked the conference authors to submit viewgraphs for immediate publication on the JCF-2 web page. This is most welcome. It is wonderful that people all around the world can read the abstracts and look at the

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A Letter from Dr. Tadahiko Mizuno to Jed Rothwell

I have good news from Dr. John O'M. Bockris. He has made a remarkable recovery from the disease after a successful operation! He was very grateful to his many old friends for the greeting card we sent from the recent meeting in Sapporo. He has no overt cancer now after operations and radiation treatment. I will hope that nothing further develops. As far as his energy and initiative were concerned, let us say they are about 75% of normal.

We are having a very difficult time working in the field of low-energy nuclear reactions. Bockris offered some suggestions, as follows.

One cannot make progress in scientific work, as he most unfortunately found, unless one has political savvy. He advises us to form some kind of alliance of the outstanding cold fusion researchers around the world. He would think of Miles, McKubre, Dufour, Hagelstein, Dash, Sinha (now at MIT), Violante, Takahashi, Mizuno, Hora, Miley, and Chubb. There are good people like Celani in Italy and some excellent people in Russia. Why don't you try to make a kind of world club of these people and see if we can't—working together—obtain funds?

As to the scientific objectives, there is one overwhelming one which would change everything, and that is *The Reproducible Experiment*. Reproducibility as he defines it means an experiment that can be done by other people when we give them the apparatus and give them the instructions, and they reproducibly produce heat at least 100% in excess of the classically measured heat.

But of equal importance would be an experiment in transmutation, which gave rise to rare metals such as rhenium, apart from the noble metals where production would be valuable. If you could show that from some cheaper materials it is possible to obtain the more expensive one at least 100 ppm, or even 1,000 ppm, it might be possible to impress an industrial firm, because they could make a profit.

A third scientific objective is to work in biological transmutation. Bockris has recently written an article about this with Mallove.

Low-energy nuclear reactions are universal throughout nature. (He would never use palladium; everyone is fed up with it.)

The third way to success is to decide on a theory, because nobody will accept anything in cold fusion unless they have a working model. By this he means that they must have a way of picturing what happens, whereby they can imagine the reaction occurs. At this point, we will begin to convince people. Which theory should we settle on? There are hundreds of theories of cold fusion, but for his money the emphasis is always going to be on neutrons, neutrons, and more neutrons, because directly after one introduces neutrons into the lattice, then one can understand transmutation and so on. Further, there has to be a key difference between low-energy nuclear reactions and fusion reactions taking place at very high temperatures, because according to the theory of these reactions (and that is what caused all the trouble at the beginning, because no one could understand how it could take place) there is no possibility of getting cold fusion at all. One has to invent a model which people can understand qualitatively, and this model has to make one realize that confinement in a metal is the key point. That, of course, has been overlooked by the classical nuclear physicists because they all think of a gas phase plasma. (Note that I have a paper—*J. Electrochem. Soc.*, **136**, 2512, 1989, in which Bockris examined a plasma type mechanism for the dielectric breakdown of water. The authors of the paper are Marek Szklarczyk, Ramesh Kainthla, and John O'M Bockris. The paper might be interesting to us, particularly because of the semiconductor model for water.)

He hopes these remarks are of value to us. He thinks, particularly, we should try to make an alliance with Miley because he really is the person in the United States who has the trust and confidence of the nuclear community. He believes that were we to be able to make an interaction (collaboration, meetings, etc.) with George Miley, it would be helpful.

Please circulate this letter to our colleagues.

Sincerely,

Tadahiko Mizuno

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viewgraphs. Someday, conference organizers will make videotapes of lectures and upload them onto the Internet, so that anyone, anywhere will be able to see the conference, months after it happens.

Several viewgraphs have been uploaded, but unfortunately at the time of this writing the ones from Mitsubishi have not. I asked Iwamura whether he intends to publish these results. He said they said they are thinking about publishing in a major journal such as *Nature* or the *Japanese Journal of Applied Physics* (*JJAP*). *Nature* would turn them down, but the *JJAP* might well publish this work. It would be a major addition to the literature on cold fusion. I asked Iwamura if there is opposition within Mitsubishi toward this research. He said that he has the support of his own upper management, but others in the company think the research is controversial and even embarrassing, and they want him to keep a low profile.

A Curious Digression into the Mind of a Skeptic

In conversation Iwamura laughed and said, "You know, people have accused us of 'faking' isotope shifts. I don't know how anyone could fake such a thing, but if we have, it is an amazing accomplishment!" The U.S. government sells mono-isotopic elements in milligram lots. A conspiracy theorist might imagine that a team of Mitsubishi scientists under the watchful eye of top management at the company's premier research establishment has spent seven years purchasing these materials and carefully blending them into the surface of palladium, and by a fantastic coincidence the element type, proportions, and distributions of isotopes they selected are similar to "fake" cathodes produced by Bockris, Miley, and Mizuno. One extreme skeptic—a graduate student in physics at the University of Maryland—told me that he does believe Ohmori fabricates cathodes with materials from Los Alamos! I asked how the transmuted material comes to be concentrated on the gold around the areas where the metal was vaporized and exploded from within. He decided that Ohmori has developed a machine to fire microscopic samples of transmuted material onto the gold surface, creating tiny holes with transmuted elements lying around them. The holes would be punched inward, like the ones Ken Shoulders observes, rather than exploded out. As a practical matter, building such a machine would be far more difficult and expensive than doing a cold fusion experiment, and it is hard to imagine what purpose it would serve. I told the skeptic that I personally have observed the "machinery" used to make these gold cathodes. It is nothing more than a simple electrolysis cell, and Ohmori does not have enough money to buy a computer, never mind a custom-built accelerator designed to bombard gold with microscopic specks of material in a way that makes the impact holes look inside out. The skeptic said that Ohmori probably has the machine hidden somewhere. I suppose Ohmori might be a character out of a James Bond movie, with a secret underground laboratory and command post staffed by three hundred uniformed lackeys, and he might be embarked upon a nefarious plot to take over the world by convincing national leaders he will transmute the gold in Fort Knox into other elements, thus destabilizing national economies. But this seems unlikely.

News of Mizuno and Ohmori

On Monday morning, Mizuno demonstrated his glow discharge excess heat experiment to Celani and me, and in the afternoon he showed it to other researchers. There is only room for three or four people in his laboratory at one time.

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On Tuesday we discussed the data in more detail, and I spent three hours observing one of Ohmori's runs (the results of which are summarized in the table below), which differ in many ways from Mizuno's. He achieved in this experiment an excess energy of 30.9 KJ, or 34.3 watts average over the 900 second run. Output was roughly 130% of input. Ohmori has seen it as high as 300% (output three times input).

Analysis of Ohmori Experiment

Input:
160 V • 0.67 A = 107.2 W
107.2 W • 15 minutes = 96.5 KJ
More conservatively, assume input power was 0.7 amps.
Total input would be 100.8 KJ

Output:
To vapor: 27.4 g • 539 calories per gram heat of vaporization • 4.18 conversion factor = 61.7 KJ
Heat loss from cell wall: 4664 J/min * 15 minutes = 70.0 KJ
Total enthalpy: 131.7 KJ

Ohmori's calorimetry and equipment is extremely simple—almost embarrassingly primitive. Ota and the NHE have spent $100,000 on a calorimeter; Ohmori uses mercury thermometers and a pencil, basing his method on junior high school level physics. Ohmori's attitude sometimes seems almost lackadaisical, almost incurious. To trigger a glow discharge you apply a burst of power. Once the glow turns on, power drops back. During the test I observed, the power supplies did not produce enough electricity to go over the hump into glow discharge, even though these same power supplies have been used successfully many times. Ohmori took this in stride, shrugged his shoulders, and said, "I can't imagine why it isn't working, but why don't you run next door and borrow another power supply?" He seemed unconcerned about this change in performance, which I imagined might be caused by some critical difference in materials or conditions. Ota and the NHE have spent $100,000 on a calorimeter; Ohmori uses mercury thermometers and a pencil, basing his method on junior high school level physics. Ohmori prefers old-fashioned, simple, pre-computer era equipment. He might use it even if he had unlimited funding. As Mizuno says, Ohmori is an analog man in a digital world.

The reader might get the wrong impression from this description. Ohmori's attitude and the care he brings to his research cannot be judged by his casual demeanor or his choice of tools. There are extenuating circumstances. The first extenuating circumstance is money. Most professors at Hokkaido University oppose this research. They will not allocate funds for Ohmori, and they will not allow him to work in his own laboratory. He has been forced to move the equipment to a filthy room deep inside a nearly deserted building. He cannot afford to buy the materials, power supplies, sophisticated computers, meters, and other data collection equipment. He cannot even afford to repair the ancient pen recorder he has used for many years. On the other hand, Ohmori would not use precision equipment even if he could afford to buy it. He thinks too much precision is a distraction, and he says people like Ota are too hung up on calorimetry, which distracts them from the real problems, which are electrochemistry and materials. Instead of improving the instruments to sense minute levels of excess heat, you should improve the techniques and materials and boost the heat up to 150% or 300%, making it easy to measure. Ohmori feels that his own excess energy is so large any method will work, and as a general principle, when you have a choice

"When they start setting up beautiful shining labs like at the NHE or in the movies, with plenty of shelf space, carefully labeled equipment, hot and cold running lab assistants, and all the rest, it will be time for us to bow out. We are only interested in the kind of research nobody else wants to do."

of methods you should select the easiest and most fundamental. Sophisticated instruments are subject to sophisticated problems, and experts can argue about whether they are working correctly or not, but no one can doubt a temperature measured to the nearest degree with two mercury thermometers. (Mercury thermometers and manual data collection are only practical for short experiments. With experiments that run around the clock, reliable, unattended computer data collection is used, which lets the experimenter have a life.)

Ota was asked why he does not boost the heat rather than improve the instruments. He replied, "I don't know how to boost it, and no one seems able to tell me." He wants to continue with the CETI-style nickel light water experiments rather than trying a radical new departure such as glow discharge. Although I think he is doing a poor job of replicating CETI, when he substitutes solid nickel pellets for thin film beads, he does have a good point. CETI has been uncooperative. They discourage replications, even threatening legal action when scientists attempt to replicate from their patent. One researcher who wanted to replicate ended up asking an expert who had not read the patent to fabricate the beads. At the ANS conference, George Miley said that the University of Illinois will not replicate the CETI design because of the possibility of CETI threats. They discourage replications, even threatening to sue people who try the experiment! Other researchers who have reported success with nickel have also failed to describe enough details. Perhaps Yamada can assist Ota with scratched nickel plates.

The second extenuating circumstance was that this was a learning session for me, not a formal experimental run. We recorded the data on scratch paper instead of a lab notebook. I moved things around, pushed and poked, adjusted the power settings to see the color of the glow discharge change.

Despite their rumpled appearance, easy-going demeanor, and the piles of broken equipment and clutter in their laboratories, Ohmori and Mizuno are crack electrochemists, who take fanatical care where care is called for. Years ago, when I was looking at his gold transmutation experiments, Ohmori brought out a wooden box containing dozens of small plastic boxes, each containing a gold cathode. I looked closely at one and asked "Is this unused, or did you wash it?" The gold surface was pristine, shining as if the sample had just been washed in acid and rinsed in pure water. He said no, that sample was electrolyzed for ninety days and placed in the box as is. There was an experiment under way in the corner, which had been running for a month. I looked at it closely and saw that the gold cathode was pristine. In an electrochemical cell, all the contam-

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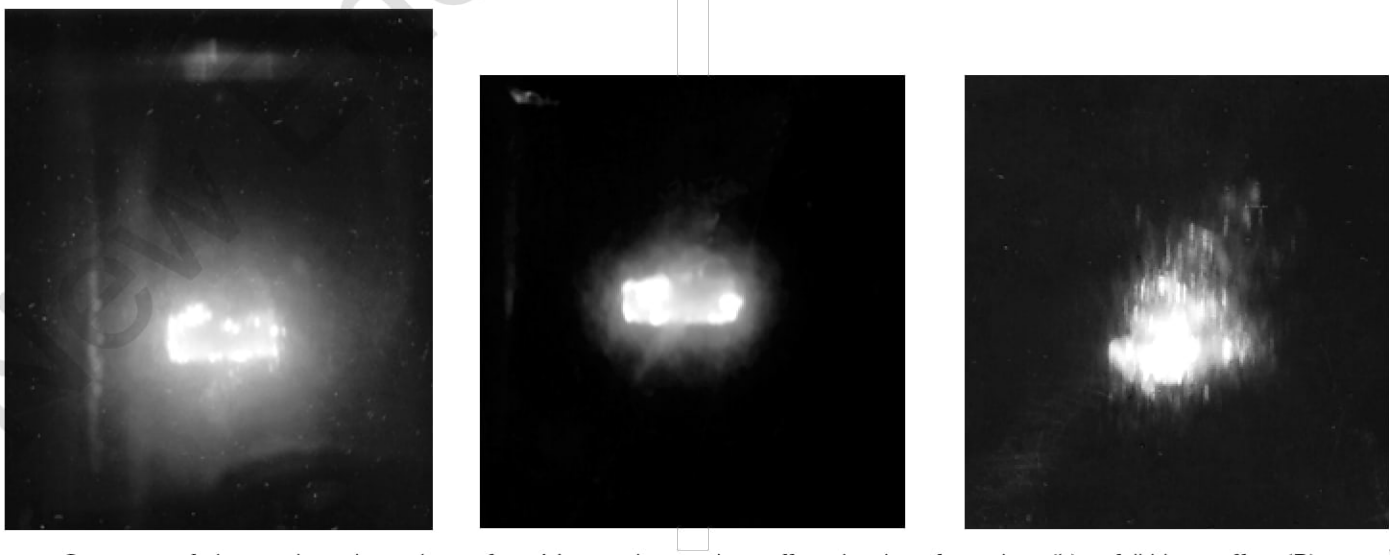
ination in the electrolyte is concentrated on the cathode surface. It is galvanized onto the surface, the way industrial machines permanently galvanize materials onto metal. The best method of removing contamination from a sample of liquid is to subject it to electrolysis and transfer the contamination to a "getter" cathode. I have seen used cathodes from many labs. No matter how much care the researchers take, a cathode left in solution for months usually comes out tarnished with a gray film of contamination, but not the ones in Ohmori's cells. Although he works in a dusty, crowded, dank building that has not been properly cleaned for years, with mold growing on the walls, when he prepares a cell using triple distilled water and ultrapure reagents, he does a better job of preventing contamination than other people manage to do in better equipped laboratories.

People like Ohmori and Mizuno are resourceful. Mizuno has what must be the last surviving model of a 1960s vintage SIMS mass spectrometer. When it breaks, as it often does, he scrounges a part from some other broken machine and fixes it himself. This is why productive laboratories are filled with piles of broken equipment.

Mizuno came to watch the last phase of the run. He held back at the door muttering: "That looks dangerous. The ultraviolet light could hurt your eyes. You need to put the cell in a proper shielded container, not in the middle of the table. I don't like the way he does this . . ." The experiment is a spectacle. It is a violent, lively process, probably as fraught with danger as it looks. The glow discharge flares, pops and sputters, making a racket. The electrolyte, which is poisonous, roils and boils out of the cell. The glass cell vibrates and bangs occasionally, disturbing the digital electronic weight scale it sits on. The cell is set inside a metal ring because occasionally it vibrates so much it threatens to fall off the scale. (The ring is larger than the cell and does not normally touch it, because this would interfere with the weight measurement.) Ohmori's colleagues asked him to move this experiment out of the regular lab ostensibly because it might produce neutrons which would endanger the students. They are hypocrites; they deny that cold fusion exists, so how could it produce neutrons? On the other hand they should be concerned about poisonous chemicals, 3000 degree plasma that melts tungsten, and beakers that sometimes vibrate so much they threaten to dance off the platform and smash onto the table.

The test run concluded without incident. We measured the heat decay curve for a while, gathered up our notes, and Ohmori gave me an unused cathode as a souvenir. It looks stronger and more elegantly assembled than the ones Mizuno makes, with a better lead wire attachment. The three of us walked out of the building, passing through rooms full of abandoned equipment from a plasma fusion experiment that must have cost hundreds of times more than the University has allocated for cold fusion, past student experiments made from jury-built equipment attached to the power and gas lines. There were steel drums, Styrofoam contraptions, and control electronics boxes with loose wires hanging out and instructions written in magic marker: "Max power setting HERE, do not exceed!" As we left the building I said, "This does not look like the setting of seminal twenty-first century science, and one of the most important physics experiments in history. It looks like a junkyard! Doesn't it upset you, the way you are treated?" They laughed, and said, "When they start setting up beautiful shining labs like at the NHE or in the movies, with plenty of shelf space, carefully labeled equipment, hot and cold running lab assistants, and all the rest, it will be time for us to bow out. We are only interested in the kind of research nobody else wants to do."

I asked Mizuno whether he has made important progress since I visited last year. He feels he has, mainly in learning to recognize what kind of plasma will produce excess heat. He took three photographs of the plasma, which he displays during his lectures. (It is difficult to photograph plasma because it keeps changing shape and color; it is like trying to photograph lightning.) In the first shot, the plasma has just formed. It is cold and small, with a few white sparks around the edge of the cathode. The calorimetry registered no excess heat when this picture was taken. In the second shot, the color has changed and the white sparks along the edge have increased, but there is still no excess heat. In the third shot, white plasma envelops the entire surface of the cathode, and showers of sparks arc out dramatically. Copious excess heat is measured when the cathode is in this state. Recognizing this state is one thing; knowing how to make it come about is another. I am not sure how Mizuno does it, although he explained and demonstrated. There are only two controls: the power supplies, and cooling water flow which raises or lowers the cell temperature. Mainly, he makes minute adjustments to the voltage, while watching



Sequence of plasma electrolysis photos from Mizuno showing how effect develops from short (L) to full blown effect (R).
Photos: Courtesy Dr. T. Mizuno.

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the cell and listening to the noise it makes. I think he works intuitively, which makes it difficult for him to help other researchers like Scott Little, who want to replicate. Watching him is rather like watching a chef melt sugar for a caramel topping: he jiggles the pot, adjusts the flame. One moment you are looking at white sugar, the next it turns to brown liquid. Try it yourself and the sugar goes up in flames.

It is much easier to observe the plasma in Mizuno's present setup because he removed the blue insulation that surrounded the cell, and he went back to using flow calorimetry to remove most of the heat. I asked him what inspired him to install transparent equipment and watch the plasma closely. He said, "It was my assistant Ms. Kawasaki. She kept asking me 'What does it look like?' 'What is happening in the cell?' and I started asking myself the same thing. Those questions turned out to be the right ones." Mizuno never hesitates to give credit to his students and colleagues for good ideas.

As mentioned above, Mizuno thinks that the cells generate dangerous levels of ultraviolet light. He wears glasses with ultraviolet filters, and he strongly recommends that other people doing glow discharge electrochemistry take all due precautions.

Mizuno's flow calorimetry is much better than it was when he used this technique a few years ago, thanks to a corporate sponsor who helped pay for and install a new set of computers and instruments. It is a high precision combined flow and isoperibolic calorimeter. It is easier to use, because it is transparent (as noted above), and because results are displayed on the computer in real time. Previously, he could not be sure of the excess heat until an hour after the run finished. He generates excess heat more reliably, at enhanced power levels, in a cleaner cell. Until recently he had a high precision power meter. It was borrowed from another department, which needed it back, so he purchased the latest model, a Yokogawa PZ4000, with awesome performance and a $12,000 price tag. Mizuno paid for this out of his own pocket. (See http://www.yokogawa.com/tm/Bu/PZ4000/Welcome.html.) He was relieved to see that it confirms his other meters to within a fraction of one percent. It records power fluctuations on a much finer scale than any previous instrument he has used, and he expects it will reveal new aspects of the reaction. He has been having problems with large bursts of RF energy, which paralyze the Agilent data acquisition switch unit. The unit often stops working when intense reactions occur during the most interesting phase of the experiment, and it takes two minutes or so to recover, leaving gaps in the data. Experts in instrument noise from the university electrical engineering department and the corporate sponsor are working to fix this.

Mizuno *et al.* published a paper about the glow discharge excess heat and transmutations in the English edition of the *Japanese Journal of Applied Physics (JJAP)* (http://www.jjap.or.jp/index.shtml). This is Japan's most prestigious scientific journal, and reportedly the second most widely cited journal in the world after *Nature*. The abstract is available on line. The title is:

Tadahiko Mizuno, Tadayoshi Ohmori, Tadashi Akimoto and Akito Takahashi, "Production of Heat during Plasma Electrolysis in Liquid," *Jpn. J. Appl. Phys.*, Vol. 39 (2000), 6055-6061, Part 1, No. 10, October 15.

The *JJAP* has published several important papers on cold fusion, including six in the July 1993 issue. Yes, despite the disappearance of the ill-fated NHE program in Japan, cold fusion is alive and well—and growing in new directions.

Second Annual Japan Cold Fusion Society Conference (JCF-2) Presentations

JCF2-1. M. Fujii *et al.* (Yokohama N.U.): Heat Measurement During Light Water Electrolysis Using Metal Rods Cathode

JCF2-2. R. Notoya (Hokkaido U.): Gamma-spectra Observation during Electrolysis in a System of K_2CO_3 Light Water Solution/Pt Electrodes

JCF2-3. H. Yamada *et al.* (Iwate U.): Low Excess Heat Evolution and Impurities Production in Light Water Electrolysis

JCF2-4 K. Yabuta *et al.* (Osaka U.): Measurement of Excess Heat and Nuclear Products by Using a Closed D_2O Electrolysis System

JCF2-5. H. Kudo *et al.* (Yokohama N.U.): Precise Heat Measurement of D_2O Electrolysis

JCF2-6. H. Numata (Tokyo Inst. Tech.): *In situ* Potentiometric, Resistance, and Dilatometric Measurements of Palladium Electrodes During Repeated Electrochemical Hydrogen Absorption; See also: Fusion Technology, Vol. 38, September 2000, p. 206.

JCF2-7 T. Ohishi *et al.* (Osaka U.): Studies on Nuclear-Reaction-in-Solid Using Plasma Electrolysis

JCF2-8 T. Mizuno *et al.* (Hokkaido U.): Heat and Other Products Induced by Plasma Electrolysis

JCF2-9 T. Ohmori *et al.* (Hokkaido U.): The Nuclear Transmutation Induced by a Plasma Electrolysis in Light Water Using Tungsten and Rhenium Electrode

JCF2-10 A. Takahashi *et al.* (Osaka U.): Possibility of Fission-products by Multi-photon Excitation Process for A>100 Nuclei

JCF2-11 M. Ohta *et al.* (Osaka U.): Analysis on Fission of W, Au, and U by LEPF/SCF Model

JCF2-12 N. Yabuuchi (Inst. High-Sci. Res.): Fusion and Fission in Metal

JCF2-13 M. Ohta *et al.* (Osaka U. and CFRL): Possible Explanation of He-4 Production in Pd/D_2 System by TNCF Model

JCF2-14 A. Arapi *et al.* (Iwate U.): Nuclear Reaction in Deuterated Palladium Electrodes Under DC Glow Discharge.

JCF2-15 S. Narita *et al.* (Iwate U.): Detection for Nuclear Products in Palladium Deuteride/Hydride in the Evacuated Chamber

JCF2-16 T. Itoh *et al.* (Mitsubishi H.I.): Analysis of Nuclear Products Obtained by Continuous Diffusion of Deuterium Through Multi-layer Palladium Containing Low Work Function Material

JCF2-17 M. Sakano *et al.* (Mitsubishi H.I.): Time Dependence of Nuclear Products Induced by Continuous Diffusion of Deuterium Through Multi-layer Palladium Containing Low Work Function Material

JCF2-18 T. Hanawa (Osaka U., Emeritus): Effect of Water in Biological Nuclear Transmutation

JCF2-19 F. Celani (INFN-LNF): Problems Related to Presence of New Specific Bacteria (genere Ralstonia and Strenotrophomonas) into D_2O Interesting with Overloading of Deuterium into Pd by Electrolytic Procedures

JCF2-20 S. Uneme *et al.* (Osaka U.): Studies of Nuclear-reactions-in-solid in Titanium-deuteride under Ion Beam Implantation

JCF2-21 Y. Katayama *et al.* (Osaka U.): Studies of Nuclear-reactions-in-solid in Titanium-deuteride under Ion Implantation

JCF2-22 T. Hayashi *et al.* (Osaka U.): Charged-Particle Spectroscopy with Decreasing Pileup of D-D Reactions for Titanium Deuteride under Deuteron Implantation

JCF2-23 H. Mori *et al.* (Osaka U.): Studies of Nuclear Reactions in Solids under Electron Beam Irradiation to Metal Deuteride

JCF2-24 K. Tsuchiya (Tokyo N.C.T.): Effects of the Bose-Einstein Condensation to the Nuclear Reaction in Solids

JCF2-25 H. Yamamoto: A Catalytic Role of Atomic Oxygen on Anomalous Heat Generation Induced in Proton Conductive Ceramics under Hydrogen Atmosphere

JCF2-26 H. Kubota: On the Possibility of Nuclear Force Attenuation by the Electric Field in the Nuclear Transmutation Reaction

JCF2-27 T. Sawada (Nihon U.): Catalyzer of the Nuclear Cold Fusion Reaction

New Energy Times Archive

Two Cold Fusion Conferences in the Washington, D.C. Area

By Jed Rothwell

The American Nuclear Society (ANS) met in Washington, D.C. at the Marriott Wardman Park Hotel November 12 - 16, 2000. Hundreds of society members attended. Most of the sessions were devoted to conventional fission reactor technology, covering topics such as "nuclear plant instrumentation, control and human machine interface technologies (NPIC&HMIT)."

The conference included a half-day session on November 15 devoted to cold fusion and a full day session on November 16. About forty people attended. These sessions were organized by *Fusion Technology* editor George Miley. This has been one of the few peer-reviewed journals in the United States that continues publishing cold fusion papers. Miley has been praised by some, but harshly criticized by others in the hot fusion community, for allowing these papers. He is retiring from his post as editor in July 2001, which could mean that cold fusion papers will no longer be published in this journal. Miley says that the incoming editor, Nermin Uckan of Oak Ridge National Laboratory (ORNL), has expressed concern about the opposition created by the cold fusion papers, but she is still considering the situation. She should announce a decision in a few months. Miley suggests that interested members of the cold fusion community might want to contact her to express views about the role *Fusion Technology* can play by continuing publication of these papers.

Since several cold fusion researchers were scheduled to be in Washington to participate in the ANS conference, Thomas Valone of the Integrity Research Institute (www.integrity-research.org) decided to take advantage of their presence to organize an informal seminar on Friday, November 17, called the Low-Energy Nuclear Reactions Educational Workshop 2000 (LENREW). The ANS speakers repeated their presentations at LENREW, which attracted a different and diversified crowd, presumably because it was much cheaper than the ANS. About fifty people came, including a surprising number of mainstream senior scientists. The meeting was held at the Holiday Inn in nearby College Park, Maryland.

About half the presentations were devoted to theory. Here are some impressions of the experimental papers.

Jacques Dufour (Laboratory for Nuclear Science, Paris) discussed experiments that apparently speed up natural fission reactions in uranium, similar to nuclear waste remediation experiments that various people have described.

G. Goddard and John Dash (Portland State University) discussed a similar electrochemical experiment in which the natural radiation of uranium is sped up. Uranium oxide (U_3O_8) is deposited on nickel cathodes, and then subjected to electrolysis with acidic electrolyte and a platinum anode. Counts are binned into three-minute increments. The experiment lasts two months. Over the first month, counts increased from 3000 to 3800 per three minutes, and they remained at that level. Unused samples from the same batch of U_3O_8 did not change. The difficult part of performing similar "nuclear remediation" experiments has been ensuring that the uranium has not been plated out inside the device or otherwise lost to the environment, but that would decrease the counts, not increase them.

Michael McKubre (SRI International) described helium findings in the Case experiments, in a lecture similar to the one he gave at ICCF-8. He began with his boldest conclusion yet about the nature of cold fusion. It is definitely "fusion; a D-D reaction resulting in helium and occasionally tritium." I found this disconcerting because a few weeks earlier, after listening to Iwamura describe heavy metal transmutations in palladium cathodes, I concluded once and for all that cold fusion cannot be anything remotely like fusion. It must be more like cold fission. McKubre's judgment is well ahead of my own, but I do not understand how he explains the discrepancy between his hypothesis and Iwamura's results.

Melvin Miles (NASWC Weapons Division) gives such understated lectures that it is easy to overlook how important and carefully thought out they are. At the ANS meeting he spoke mainly about palladium boron cathodes, which were fabricated by metallurgist M.A. Imam of the Naval Research Lab (NRL) in Washington. Miles achieved 100% reproducible excess heat with these cathodes. Eight out of nine produced excess heat, and the ninth failed for reasons that are now clear and can be prevented. He also achieved perfect reproducibility with the special Johnson Matthey palladium, but this is a dead-end approach. Johnson Matthey will not reveal how it makes its palladium, supplies have run out, and they express no interest in making more. Imam knows exactly what steps he took to make the cathodes. He is willing to discuss them in detail. He did extensive studies of the finished material characteristics, measuring composition, grain size, and so on. He would be willing to make more if the Navy would permit him to do so. The Navy has ordered Miles and Imam not to do cold fusion research, although Miles is allowed to use lab facilities on his own time, on weekends.

Miles used the NRL palladium-boron alloy in experiments in his laboratory at China Lake, and in Japan at the NHE. Although he saw higher excess heat from the special Johnson Matthey cathodes, he achieved the highest success rate with the NRL boron alloy. Some people have suggested the boron takes part in the nuclear reaction. Miles does not agree. He thinks the boron has the same effect as the cover gas used

Dr. Mitchell R. Swartz (L) and Tom Valone (R) at LENREW 2000. *Photo: E. Mallove*

New Energy Times Archive

while fabricating the Johnson Matthey cathodes. Both keep oxygen out of the finished metal. The boron oxides are easily separated from the molten palladium. In the finished composition the percent of boron was slightly reduced from the percent of starting ingredients. Another reason boron helps could be that the boron alloy loads and deloads more slowly than pure palladium, so it holds the deuterium in place long enough for a reaction to occur. The alloy may prevent deuterium from dispersing through the bulk of the metal, holding it at the surface where the reaction occurs. This is the opposite of the "high loading" hypothesis which has dominated the field.

Miles showed three graphs of his NHE temperature data: as analyzed by his own equations; by Fleischmann's equations; and by the NHE method. The NHE claimed that Miles did not observe excess heat, but only large perturbations above and below the zero line. Miles and Fleischmann believe the NHE reached this conclusion because they used only one calibration point, from a 0.2500 watt heat pulse on day three of the run. They did not realize that the system was already generating excess heat when this pulse was applied. This mistake shifts the rest of the data down by about half the value of the excess heat, so that instead of going 200 milliwatts above the zero line, back down close to zero, and then up about 200 milliwatts again, the NHE graph shows the heat level wandering up 100 milliwatts and then down a negative 100 milliwatts below the zero line. No endothermic reaction was possible at that stage: negative heat has to be instrument noise, so the NHE concluded that the noise level is approximately 100 milliwatts, and the excursions above and below the zero line are meaningless.

Miles, Imam, and Fleischmann are writing an extensive report about the NHE experiments which will be published by the NRL in a few months. This is big news—Martin Fleischmann's name on a U.S. government report.

Tadahiko Mizuno (Hokkaido University) described the plasma electrolysis results covered in the recent paper in the *Japanese Journal of Applied Physics*. (See "Report on the Second Annual Japan Cold Fusion Society conference.") He showed some data taken with his improved instrumentation and the Yokogawa PZ4000 power meter.

John Kenny (Bradley University) described an interesting little experiment designed for undergraduates, with an innovative cell design. The purpose of the experiment is to measure increased beta counts from palladium cathodes using a Geiger counter. A specially designed L-shaped cell holds about a milliliter of electrolyte. A platinum anode is inserted at the top (the vertical portion), and a specially made palladium cathode is fitted into the bottom (the horizontal part of the cell). The palladium cathode acts as a stopper. It keeps the electrolyte from leaking out, and part of the palladium protrudes from the bottom of the cell. A 1 mil thick copper plate is pushed against the bottom of the palladium with a spring. The cover plate acts as the lead wire, conducting electricity into the cathode, and it also acts as a shield to filter out radiation from tritium, which may be present in the electrolyte. The cathode is small, so it soon loads with deuterium. After about fifteen minutes it apparently begins reacting. A Geiger counter is placed next to the copper, and another one is placed some distance away in the room to measure background. In some cases, the second Geiger counter is placed next to another cell also undergoing electrolysis, with a dud palladium cathode (defined as a one which has never produced an increased count, or which worked for a time and then mysteriously stopped). The foreground and background counts are binned in 10 minute increments, and the rates are compared. The Geiger counter next to the cell usually shows higher counts for each 10 minute segment, and for the entire experiment it consistently shows significantly higher counts. The difference may not be statistically significant; it is only one or two sigma at best. The Geiger counters are thirty years old and not very sensitive. The experiment is now being reproduced at the University of Illinois with more advanced instruments. Preliminary results show a slight increase over background.

The experiment takes only two hours and it always works. The two sigma results are not convincing, but perhaps they can be improved enough to impress many people. The cell design, with one surface of the palladium actually outside the cell, eliminates the water and glass barrier which probably stops weak radiation and X-rays. It is similar to the diaphragm cathode used by Iwamura, which has gamma detectors mounted below the exposed lower surface.

Richard Oriani (University of Minnesota) described a mind-boggling experiment. He has what looks a little like a miniature version of the Griggs HydroSonic Pump™: spinning rotors with holes in them, immersed in water. He detects bursts of excess heat, that turn on and off unpredictably. His presentation was cursory and disappointing in its absence of details. He showed hand-drawn graphs with no indication of the units or values. The heat bursts are shown as relative temperature rises, with no indication of the absolute power level. In private discussions, he said he tried to measure the heat rigorously with a Seebeck calorimeter he built himself, but he was not satisfied with the results. He has not even attempted to estimate the excess power. All he knows is that the temperature goes up when it should remain stable. Based on the motor power and temperature before the bursts, I estimated the excess heat bursts are a watt or two. His lecture and preparation was surprising because he is a consummate professional electrochemist. People who do not know him may dismiss this as a crude experiment, which is a shame, because it probably deserves attention.

George Miley (University of Illinois, Champaign-Urbana) described thin film experiments he discussed at ICCF-8. The results seem impressive, but there are problems with his calorimetry and his cathode-anode geometry. The calorimeter is so well insulated it takes days to establish a single calibration point, so only a few calibration points were taken, which is not enough. What is worse, the excess heat was sometimes below the lowest calibration point. The geometry of the glass slides probably causes recombination. The anode is immediately below the cathode. At the low power levels used in these experiments, bubbles of oxygen will slowly form on the anode, and then float straight up to the cathode where they will mix and recombine with hydrogen. The report shows that even with 100% recombination, a little excess heat would remain, but the uncertainty of the calibration

Dr. Tadahiko Mizuno, Dr. Akito Takahashi, and Dr. George Miley at ANS Meeting. *Photo: E. Mallove*

New Energy Times Archive

combined with recombination makes the results doubtful in my opinion. Miley recently shipped glass slide cathodes to the New Energy Research Laboratory, and we shipped them to Edmund Storms, who tested them in a Thermonetics Seebeck envelope calorimeter. Storms did not observe excess heat (see page 21, Device and Process Testing). Miley thinks this is because the thin film cracked and delaminated, which did not happen with the glass slide cathodes tested at the University of Illinois. He suspects the glass surface was made too rough before the thin film was applied. This left hills and valleys forming sharp protrusions which put too much stress on the thin film. He hopes to make replacement cathodes, which he will send to Storms for testing.

Miley showed slides of the thin film plated beads that the University of Illinois fabricated in collaboration with CETI, and he said the film adheres well because it "holds onto itself." He believes this geometry is superior to glass slides, because it is less likely to delaminate. I asked why he does not use beads. He said it was because CETI has threatened lawsuits for patent violation.

Mitchell Swartz (Jet Energy Technology) described his advanced calorimetry and his optimal operating point (OOP) hypothesis. The OOP hypothesis is based on the widely observed fact that cold fusion cathodes produce the largest input-to-output ratio at certain "optimized" power levels. This is especially clear with light water-nickel. At very low power, you see no excess heat. As you gradually increase power, the heat appears, and it reaches an optimal peak. You can raise the input power higher, but the excess heat does not increase, and it may actually decrease. The extra input energy does not improve the conditions needed to cause cold fusion; it is wasted. Swartz thinks this phenomenon reflects some important aspect of the nuclear effect, and that by carefully measuring where the optimal output occurs, he may learn to improve the ratio enough to make practical devices. No one disputes these observations, but other scientists think the operating peaks are caused by mundane electrochemical processes, with no special significance and no long-term importance to the development of technology. They assume that practical cold fusion devices will have input to output ratios thousands of times higher than today's experimental devices, and they may use entirely different methods of loading hydrogen into the metal. Swartz claims that he has already made significant progress toward practical devices. He has measured spectacular excess from a variety of cathodes, including some combinations that other people have not tested, as far as I know. His highest input to output ratio occurs with nickel cathodes and gold anodes. In his published papers he reports excess heat at ten times input, and during the lecture he said results are as high as thirty times input, and he has even progressed to the point where he converts some of the excess heat into electricity. If this is true, these are among the most impressive cold fusion results ever reported. However, I doubt these claims because his calorimeter is custom-built, and he claims extraordinary levels of precision and accuracy for it. Whenever possible, you should use standard, off-the-shelf equipment, not an exotic one-of-a-kind instrument. Otherwise the instrument itself becomes the focus of attention and a legitimate source of doubt. Swartz's excess power is above a watt, so it could easily be measured with ordinary equipment.

Hal Fox (Fusion Information Center) described what I consider to be a mishmash of over-unity energy devices, including cold fusion-like claims from Mills, a high-density charge cluster device made by Ken Shoulders, and a variety of claims such as AquaFuel and a generator invented by Tom Bearden. I think such speculative claims should be presented with more doubts and caveats than Hal Fox or Thomas Valone express, and they should not be equated with the rock-solid, replicated, published experiments in cold fusion.

Despite my misgivings about some of the presentations at both the ANS and LENREW meetings, it is good that there are forums between the major ICCF's where cold fusion and other over-unity claims can be discussed openly.

MEG: Over-Unity Motor Breakthrough?

Tom Bearden has long been regarded in the new energy field as a prominent theorist (see his article in *IE* No. 5/6, "The Master Principle of EM Over-unity and the Japanese Over-unity Engines," pp. 38-55.) This fall, his experimental group in Alabama caused a flurry of interest in an electromagnetic motor dubbed the "MEG" ("Motionless Electromagnetic Generator"), which he asserts has already demonstrated a performance of 96 electrical watts output, 6 watts input, *i.e.* a COP of 16—with claims that they have gotten even higher power ratios.

The group has formed a Nevada-based corporation, Magnetic Energy Limited, to deal with this technology. According to Bearden, patent applications have been filed both in the U.S. and abroad. A respectable university electrical engineering department has been engaged to provide an independent replication and definitive test measurements, which they say should occur by sometime in January 2001. (The name of this university is being withheld temporarily though it was mentioned to this editor, provided that it is not disclosed publicly.)

A technical paper about the theory and general characteristics of this motor appeared for a short time on a DOE website: (http://www.ott.doe.gov/pdfs/MEGpaper.pdf)

According to Bearden, the paper has been temporarily removed per the wishes of the patent attorneys, but it will return there in the near future. Bearden now has his own website: (http://www.cheniere.org). J.L. Naudin in France claims to have made an independent replication of the MEG already: (http://jnaudin.free.fr/html/megv21.htm).

Jed Rothwell challenged the claim based on his view that it should be possible to make the machine self-sustain. In other words, it should be possible to take the output of the machine, store it in a battery, and when the battery is fully charged, use the battery to run the machine indefinitely. Bearden claimed that connecting the electric output directly to input would not work, but this is not the configuration Rothwell suggested. Bearden shot back on the Vortex forum with lengthy e-mails, which included this passage expressing his extreme confidence:

"We patiently explained that we are arranging to present the scientific community with an impeccably replicated MEG at a fine university under DoD auspices. Please remain skeptical until that is accomplished, but do not be dogmatic! We will also present the results of completely independent testing of the MEG by a U.S. government-certified test laboratory. Those two things you can rigorously and properly hold us accountable for. We welcome you doing so, because that is rigorously scientific and the scientific method demands it of us. If we don't do those two things, then 'you have us' firmly, and strong retractions would have to be made by us. In short, we will have rather resoundingly failed.

"However, I must confess that, with the MEG sitting comfortably in front of me with 6 watts input and 96 watts output in 110 volt lamps, measured with multiple types of instruments, I really don't have any fear of having to retract. Particularly since there are two such MEGs sitting there, both working quietly away without any moving parts. We built another successful one, and destroyed it in pushing it to see the rise in COP and its limits."

Infinite Energy will provide further details on the MEG as they become available. —*EFM*

New Energy Times Archive

New Energy Research Laboratory ✧✧✧ Device and Process Testing Update

Eugene Mallove

"Hot" Cathode Emerges

A platinum cathode in a heavy water electrolysis system has once again been made to produce excess energy (see discussion in report on ICCF-8, *IE* No. 32, p. 28). Dr. Edmund Storms reported to us recently from his Santa Fe, New Mexico laboratory that a platinum cathode was caused to make excess energy on purpose by applying a special surface coating. Three samples have been made to work at excess power levels up to 400 mW. The nature of the "active region" on the new cathode is still not understood, but it clearly is not consistent with previous experience and theories in the cold fusion field, Storms says. In private discussions with NERL, Storms said he believes the method is very reproducible and can be made to produce higher levels of excess power than he is obtaining at this time. At the moment, the power ratio is very modest. Input power to the electrolysis ranged from 10 to 25 watts when excess heat was present. Excess power is apparently proportional to current.

If this turns out to be a viable, reproducible cathode methodology, it would be expected to offer great benefits to the cold fusion field. We'll keep readers posted on what develops from this new direction.

Miley Thin Film Cathodes

Dr. Storms began testing the thin film cathodes that were prepared by Prof. George Miley's group at the University of Illinois. So far he has found no evidence of excess heat, but to be fair, the batch of cathodes seems to embody a basic problem: cracking of the thin metallic films on the glass or quartz substrate. "It is very clear to me that the films are breaking up and becoming detached from the substrate during electrolysis," Storms wrote upon viewing SEM photos provided by the Miley group. No excess power was produced over a wide current density range. In Dr. Storms' opinion, the behavior of the as-received Miley cathodes indicates that loading causes damage, which leads to de-loading, especially in the region near the anode wire. These samples all show the same basic behavior, hence they need to be replaced by samples which do not show this effect before any more studies are done.

Dr. Storms will have time to test some new samples on the road to laying to rest the influence of cracking, as found in the first batch of thin-film cathodes delivered to NERL. Storms has demonstrated that using the open circuit voltage as an analysis tool provides a quick method to identify cracking, thereby allowing him to sort through samples prepared by different methods rather easily. Using this technique, he could help Dr Miley's group identify the proper conditions so that the supplied samples would be more reliable.

Storms wrote to us:

"The main problem is not with the location of the anode, but with the characteristics of the thin films. These films are destined to come apart when they load. The end near the anode loads most, hence comes apart first and most rapidly. If loading were uniform, the entire sample would come apart at the same time. By having non-uniform loading, hopefully some part of the sample would achieve a sufficiently high loading to make heat before it also came apart. Of course, this is a bad design if the sample were expected to make heat over its entire surface.

"A basic materials problem exists. Palladium must expand when it loads. The Ni also will expand, but in a different amount. The glass remains fixed so something has to give. By making the glass rough and by making the layers very thin, expansion would not cause the layer to delaminate—this may have been the thought. Presumably, Miley has achieved success in this respect."

Dash Titanium Cathode Cell Testing

With minimal changes to its set up, Dr. Storms tested a cell that had been designed and built by Ed Wall here at NERL to reproduce the work of Prof. John Dash with titanium cathodes. Storms filed this report with us:

"I have finished the study of the Ti sample within the cell you sent. The Seebeck calorimeter was used with $D_2O+H_2SO_4$ electrolyte and this was calibrated after the Ti study using a dead Pt cathode. The results are as follows:

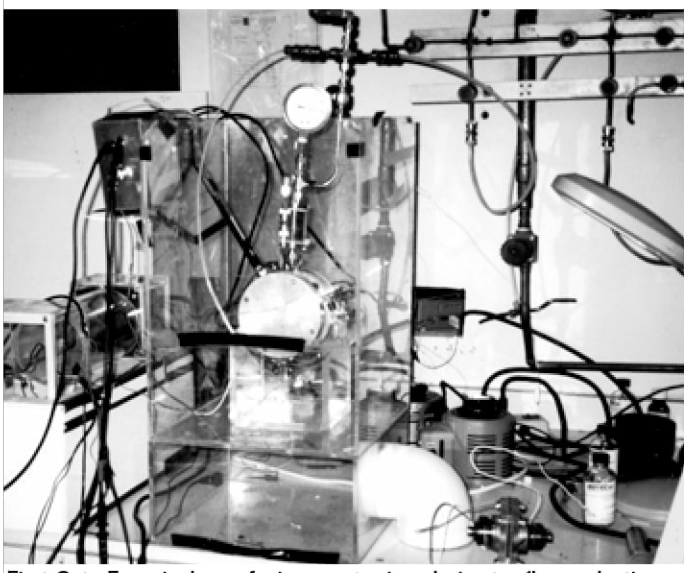
Current, A	Excess power, W (±0.05W)
0.48	-0.065
0.98	-0.022
0.18	-0.049
0.48	-0.071

"The sample lost 0.00674g (10.3%) during the study and a copper colored solid precipitate was found floating within the electrolyte. Because the cell was not gas tight, I could not measure the amount of D taken up by the sample. However, the shape of the precipitate suggests that a thin layer was removed more or less intact from the surface as a result of hydride formation.

"I believe no excess energy was made and the slight negative bias resulted because the cell had to be disturbed in order to insert the Pt cathode for calibration. This could have been avoided if an internal heater had been installed."

First Gate Energies Reactor

In November, we acquired from Roger Stringham of First Gate Energies a sonofusion reactor of the general type that



First Gate Energies' sonofusion reactor in calorimeter (large plastic structure). Apparatus to the left is oscillator transducer calorimeter.

Device continues on page 70

New Energy Times Archive

Status Reports on Technologies and Claims

This is the first in what will be a continuing series of updates on the activities and achievements of new energy companies, organizations, and inventors.

New Energy Companies and Inventors

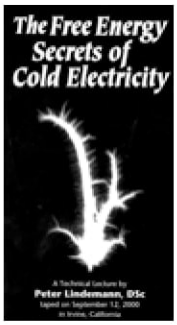
BlackLight Power Corporation
493 Old Trenton Road, Cranbury, New Jersey 08512 USA
Phone: 609-490-1090 • Fax: 609-490-1066
www.blacklightpower.com
CEO: Dr. Randell Mills
Undoubtedly at present the most active and well-financed new energy corporation. It is still privately held. Continues its work on two major fronts: 1. Energy generation from high-temperature catalyzed gas phase reactions involving the formation of shrunken hydrogen atoms and 2. Hydrino hydride chemical compounds for a variety of industrial applications, including extremely high charge density batteries. On-going battle with USPTO appears to have set back BLP's attempt to achieve IPO status under Morgan Stanley Dean Witter auspices.
See *Infinite Energy* Nos. 7, 12, 17, 23, 24, 31, 32, 33

Labofex
42 Rockview Gardens
Concord, Ontario L4K 2J6
CANADA
Principals: Dr. Paulo and Alexandra Correa
Primary technology is thoroughly patented PAGD (Pulsed Abnormal Glow Discharge) reactors that under test produce substantially more electric power output than input. Efforts to commercialize and improve the technology are continuing.
See *Infinite Energy* Nos. 7, 8, 9, 17

ENECO
391-B Chipeta Way, Salt Lake City, Utah 84108 USA
Phone: 801-583-2000 • Fax: 801-583-6245
This privately held company has put cold fusion "on the shelf," after encountering difficulties with its proprietary gas phase approaches. Dr. Yan Kucherov remains the Chief Scientist, involved with the company's advanced energy conversion technology, which the company hopes will supplant even present state-of-the-art thermoelectric devices. CEO Fred Jaeger has left the company after seven years of service. A trusted interim CEO is now in place.
See *Infinite Energy* No. 4

Clean Energy Technologies, LLC (CETI)
2033 Wood Street, Suite 120, Sarasota, Florida 34237-7926 USA
Phone: 941-951-2384 CEO: James Reding
Formerly (1995-1998) a very high profile company in the field of low-energy nuclear reactions (LENR), based primarily on the Patterson Power Cell, for which an extensive patent position (approximately fifteen patents) was achieved. Rumors of problems in repeatability of the excess heat phenomena from metallic film coated beads are denied by Chief Scientist Dr. James Patterson, who continues his work in new directions— a "different tack." Patterson says, "We took some time off to make some money, and now that we've made it, we're back at work." He expects this new LENR direction will bear fruit in about a year. Staff has been reduced considerably, but work proceeds. The company is in the excess energy and radioactivity remediation business.
See *Infinite Energy* Nos. 2, 5/6, 9, 10, 12, 13/14, 15/16

New Energy Companies and Inventors



VIDEO REVIEW

"The Free Energy Secrets of Cold Electricity"
A Technical Lecture by Peter Lindemann, DSc.
Review by Ken Rauen

Having access to Cold Fusion Technology's vast library of video tapes of new energy devices and claims, and having seen many, many tapes on a tremendous variety of topics, Dr. Lindemann's video taped lecture impressed me as refreshingly different. He talks about the experimental work of three inventors, most prominently Edwin Gray and Nikola Tesla, and a little bit about a lesser known inventor with a famous and mysterious device, the Testatik Machine. (Editor's note: the Testatik Machine was covered in *IE* No. 32, pp. 64-65, "Is This Really True?" by Lev Sapogin.) Would you believe that all three might be related, that they may be different embodiments of the same phenomenon? I was astounded to hear a well-presented argument that this could be.

Edwin Gray's self-running electric motor was only covered in newspaper articles a couple of decades ago. Those articles sounded too good to be true; one can easily take the attitude that the reporters who wrote the articles did not know what they were looking at, not being qualified to do a scientific evaluation of his devices. Gray's patents did not make sense; they did not explain what he was claiming the machines were doing. Besides, why did it all disappear? Was it real?

Nikola Tesla is an even bigger enigma, though his feats were witnessed by too many to dismiss his reputation. His solid achievements are uncontested history: the invention of radio, as established by the U. S. Supreme Court over the disputed Marconi patents, and the worldwide use of polyphase alternating current electricity. Despite the history, much of the lore about him does not make sense. Tesla demonstrated wireless light bulbs at an IEEE meeting in New York in 1888. What were these devices and what was behind the Wardencliff adventure? Tesla abandoned wireless signal transmission and was doggedly pursuing wireless *power* transmission. Tesla was on to something, and Tesla was not just a dreamer. He was a doer. Did he know something that history has not related? Dr. Lindemann puts it all in astonishing perspective. In the short span of one long lecture, Dr. Lindemann explains Tesla's bizarre electricity experiments, which no amount of exposure to Tesla's work from all other sources has ever done for me. I think he has finally figured out what that genius was up to, without plumbing the depths of that great mind.

The history of physics is even more clear now. The mid to late 1800s saw a pivotal change in the development of science. Maxwell and Heavyside developed equations describing electromagnetic phenomena; they were developed from fluid dynamics. I believe the equations and the resultant understanding of them were altered. I am not alone in this matter. Tesla sharply disagreed with Helmholz, Hertz, and others who took electricity and magnetism to where it is today. Dr. Lindemann points this out and identifies a practical avenue for the use of Tesla's understanding of electricity, which recognizes the aether as real and as an immense source of energy. In the end he explains, with the aid of previously unpublished photographs of Gray's machines, how Gray applied Tesla's artificial lightning to a practical device.

If New Energy Research Lab were not presently busy with promising projects, I would definitely attempt to build what Dr.

Video Review continued on page 40

New Energy Times Archive

A Conversation with Rashmi Mayur

by Bennett Daviss

"The opportunities for new and renewable energy technologies are mind-boggling. . ."

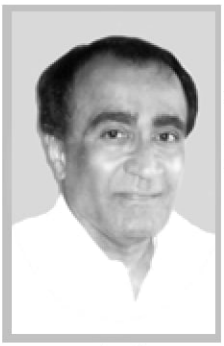


Photo: Courtesy Dr. Mayur

Dr. Rashmi Mayur

For two decades, Rashmi Mayur has been a scout on the frontier where ecological issues, energy technologies, and the future come together. As founder of the International Institute for Sustainable Future in Mumbai, India, Mayur strives to set a new direction for policymakers who venture across that complex terrain.

Mayur earned a doctorate in environmental science at New York University in 1977 and returned to his native India where he founded his institute in 1982. In the same year, he helped to form the Global Futures Network, a loose web of 3,500 scientists, planners, and public officials working to balance issues of environmental protection and economic development. In the intervening years, he's become a consultant to seven national governments, including those of Canada and Australia, and to several United Nations agencies—and often finds himself at odds with the assumptions and directions that those agencies take.

Passionate, always articulate, Mayur addresses more than forty gatherings around the world each year, ranging from the Kyoto environmental negotiations to the upcoming triennial congress of the World Future Society. In each of his appearances, his message is consistent: the need for an ethic of Third World economic development that links the needs of people to those of the natural world.

Q: There was a time when renewable energy loomed large in the future of poor countries. What happened?

A: After the oil crisis began in 1973, these governments turned to renewable energy technologies in a serious way. Through the mid-1980s, China invested in methane digesters; other countries gave support for development and installation of a variety of kinds of renewable energy. It was the thing. But when oil became cheap again, that took the steam out of renewables. Government investment declined, leaving the field to private companies—and, of course, companies cannot sell even a $50 solar cooker to an Indian villager whose income is 80 cents a day and has to feed a family of five or eight.

As a result, more than one-third of the people in poor countries now rely on another form of renewable energy: firewood. But that is not sustainable and, increasingly, not even renewable. In those countries, and particularly in tropical countries, 23 million square acres of forest disappeared in the year 2000. That means death to the farmers who clear the land to plant crops. Groundwaters are not recharged, the soil degenerates, harvests fail, and poverty worsens. Nepal has lost 80% of its forests, and the result is that every year Bangladesh is flooded. Relying on wood as fuel in developing nations is an ongoing ecological catastrophe.

Q: How big are the opportunities for renewable energy in those countries?

A: In India alone, there are 700 million people living in villages, perhaps 120 million families. Can you imagine selling 120 million solar cookers or 120 million photovoltaic systems? The opportunities for new and renewable energy technologies are mind-boggling.

During the 1980s, China installed perhaps 10 million methane digesters in farms around the country. With the waste from two cows or a few pigs, a digester can produce enough methane for cooking and perhaps an hour or two of lighting after dark.

The Solar Electric Light Fund, based in Washington, is creating market-oriented ways for poor villagers to put photovoltaic systems in their homes and farms and has done so in villages in eleven countries. So progress is being made, but it is small and much too slow.

Q: The obstacles to renewable energy aren't strictly economic, are they?

A: Renewables represent the future—decentralized, environmentally friendly. They are, in an important sense, post-industrial. But our historical and technological context leads government officials in poor countries to assume that "development" means mass-scale industrialization. They still think that industrialization is the way to national wealth and also to earn the respect of other nations. This is an antiquated notion that is irrelevant to poor countries with limited resources. It also has virtually nothing to do with improving villagers' health and living conditions and providing them the tools they need to build themselves a sustainable future.

But the man from India or Ghana looks at an electric power plant or a highway and thinks, "Oh, it is beautiful." The entire brain has been washed to believe that when you have a highway, you have the cars, the industry, the jobs, the wealth, and a wonderful life of abundance. But it is a death metaphor. You cut your forest to clear the road, divert your natural resources to build more and more cars, burn your oil to power more and more vehicles.

Q: But if you push for economic development among poor nations, why aren't large-scale power sources the fastest way to get there?

A: Building mass-scale generating plants isn't just an environmental mistake. It's a quick route to disaster in several forms.

To build a centralized power plant, you need concrete for the foundations, steel for the transmission towers, technicians to maintain the turbines and the rest of the system. There are two ways to get those materials and that expertise. One is for a country to develop those resources internally. That's a very long process and these countries have barely begun it. The other way is to go into debt to buy it all from industrialized countries.

These countries have no customer base that can pay monthly electric bills. The billions of dollars it takes to build a plant cannot be paid back through sales and profits, so it becomes a government debt. The primary resources these countries have to turn into cash to repay debt is their natural wealth. So they log their rainforests and strip-mine their minerals to raise cash. Then the only trees that are left are made of steel and have power lines hanging from them. The

New Energy Times Archive

agriculture is ruined, the climate is ruined, the land becomes desert, the people have destroyed the only way of life they know, they starve, and their society collapses.

A centralized power plant is the culmination of a long process of industrial development—which, by the way, has proven unsustainable in its present form. To expect to foster a process of development by building its end result first is completely backward.

Q: How do you distinguish between the energy technologies you favor and those you don't?

A: We can use a very simple principle to distinguish among technologies. A benign technology brings more benefit than harm over time; a malignant technology eventually brings more damage to the Earth than its benefits justify.

The technology that wiped out smallpox is a benign technology. The bicycle is one of the greatest technologies ever invented. The technologies of space hold tremendous promise for the nations of the South.

In contrast, the planet now has tipped beyond the point where technologies that burn fossil fuels justify their use. The ozone layer is being corroded away, the Marshall Islands already are disappearing under the Pacific Ocean, global temperatures are rising—and open water, a lake, has been discovered near the North Pole. All these have been shown to be the result of using unsustainable and nonrenewable energy technologies.

Q: So the uses of space represent a benign technology?

A: There is an inherent synergy between renewable energy sources and space-based telecommunications technology.

Our institute is working with a company in California to create "information kiosks" for the villages of the South. Each kiosk contains a cellular telephone, a radio, a television, a VCR, and a computer connected to the Internet, all powered by decentralized, renewable energy sources.

With information kiosks, we can transmit life-transforming information and communication abilities via satellite to the poorest and remotest parts of a country. We could broadcast information on hygiene, on improved farming techniques, on renewable energy. We could bring literacy and other kinds of education. When a person is seriously ill, a family member can telephone for medical advice.

If a farmer has a cellular telephone in his village, he can call to the market twenty miles away to learn that everyone has gone there today to sell their mangoes and that if he goes to a different village, he can get a higher price for his crop. If satellites show a typhoon forming in the Indian Ocean, we can broadcast warnings to the farmers who live in the lowlands of Bangladesh and tens of thousands of lives could be saved. The possibilities for uniting renewable energy technologies with space-based communications are endless.

Space itself also can become a source of energy. For several years, I was on the advisory board of an enterprise originated by Peter Glazer at Princeton. He developed the idea of capturing solar energy in orbiting satellites and transmitting that power back to the Earth's surface by laser beam. Then the power could be channeled through the distribution system already in place. There are still huge technological barriers to overcome, the development costs would be at least $75 billion, and it will take time. But we can weigh those investments of time and dollars against the potential benefits—the elimination of nuclear power and its problem of toxic waste, potentially huge reductions in emissions from coal and oil in power plants, attacking the problem of climate change.

Q: Using your criterion of benign energy technologies, sketch a scenario for wide-scale economic development that doesn't rely on centralized power generation.

A: Political officials in poor countries assume that economic development is a linear process: that first you are pre-industrial, then you become industrial, then you are "developed." That no longer needs to be the case. Using renewables, the nations of the South can leap over the environmentally unsustainable "industrial" phase and go directly to a cleaner, post-industrial energy economy.

Sunshine is abundant in most countries of the South. India, for example, has sunshine on 292 days in an average year. Solar technologies are simple enough and inexpensive enough to meet the basic needs of typical households in those areas.

Solar ovens use one-way glass with a mirrored inner lid to focus the sun's rays into a black-lined cooking chamber. The devices are the size of a briefcase, require no maintenance, and cost no more than US$70. Cheap solar evaporators can separate clean water from water tainted by a number of pollutants.

A $1,450 solar-powered water pump uses just 200 watts, as much as three average light bulbs, and can deliver 30 gallons of water—the minimum needed for health, as specified by the World Health Organization—for 135 people every day. Just one of those could save thousands of hours of labor each year, mostly by women and children, for people who now spend literally hours each day hauling water for their families.

Solar energies also make business possible. To find work, most villagers now must go to cities where they do manual labor, pick through garbage dumps, and live and die in slums. Most of those people don't want to go to cities; they want to stay in their villages. Renewable forms of energy can power small machines that can enable them to operate village-based manufacturing or other businesses that keep people in their homes, keep villages and cultures together, and that don't plunder the land the way that mass-scale industrialization does.

The enormous growth in micro-lending—the trend among banks in these countries to lend fifty or a few hundred dollars to poor people to start small businesses—can support that kind of enterprise.

So renewable energy technologies are a key to preserving the environment and preserving indigenous societies and cultures while bringing economic progress down to the level of individuals and villages instead of powerful elite groups of public officials and their cronies.

The technology is there. The task now is to reorient the World Bank and other international development agencies away from conventional, massive energy projects and toward a new energy economy for the nations of the South.

Q: What are your expectations for new energy technologies in development, such as those being researched under the moniker of "cold fusion"?

A: I'm cautiously hopeful, because these kinds of decentralized energy technologies could be beautifully compatible with post-industrial development. In the short term, I don't expect much to happen—not only because technological challenges remain, but also because of the structure of conventional energy's commercial marketplace. A physical, financial, and psychological infrastructure exists that will take time to redirect, if not to replace. But over the longer term, there is reason to hope.

Bennett Daviss contributes to several national and international magazines, including *Discover*, *New Scientist*, and *Air & Space*.

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Leapfrogging Traditional Technologies in the Developing World

David Hart
Imperial College Centre for Energy Policy and Technology, U.K.

Energy use in developing countries is growing faster than in any other area, and will continue to do so—both because of increasing populations and their desire for an improved quality of life. The way energy is used is also changing—from basic fuels such as wood and dung for heating to more "organized" forms of energy, such as electricity, for lighting. However, unless highly forward-looking decisions can be taken soon, this growth in energy development will be accompanied by concomitant growth in pollution and increasing emissions of greenhouse gases.

It is possible that "technological fixes" may be able to help out. Rather than develop the same large, centralized, and expensive energy supply infrastructures that have been traditional in the developed world, many developing countries can use technology to "leapfrog" to a structure that many are also suggesting might be more efficiently used everywhere—decentralized electricity. By using locally available sources of energy such as biogases, wind power, solar energy—or natural gas and oil—but designing efficient systems including energy storage, it may be possible to drastically reduce the conventional link between growth in economic welfare and rise in energy use and emissions.

Technologies such as microturbines and, especially, fuel cells and hydrogen storage, may prove to be crucial in this new era of energy. Although currently expensive, it is potentially much cheaper to put a self-contained system into a remote area than to build a large-scale power plant and a grid connection over long distances and poor terrain.

Crucially, the amount of investment required for each local system will be much less than for the large-scale centralized plant, and so financing sources may be more readily available. Demand for expensive and volatile imported fuels could also be reduced.

There is still much to be done in terms of proving new technologies in the many and varied environments under consideration, and in reducing their costs. However, institutions such as the World Bank have a strong interest in these areas, and further progress towards agreement on measures to combat climate change could unleash powerful new sources of funding in the form of the Clean Development Mechanism.

The pace of development, not only in technology but also in economic reform and in many other areas, may soon make the world's energy future much clearer.

David Hart has a MSc. in environmental technology, and a B. Eng. (Hons) in mechanical engineering. He specializes in research into the economic and environmental aspects of fuel cells and hydrogen both on an academic and consultancy basis. He has written many papers on the subject and is a member of the International Association for Hydrogen Energy and the International Association for Energy Economics.

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ALL GAS AND SANDBAGS
The UK Fuel Protest Meets a Watery End

Soo Seddon

The second week in November saw widespread flooding in the UK. Ferocious rainstorms caused many rivers to over-flow their banks, isolating whole towns and villages up and down the country. Thousands had to abandon their properties and several hospitals and nursing homes had to ship their elderly residents into church halls and community centers as the flood waters rose. Insurance claims were estimated in the £millions and, almost unnoticed amid the chaos, the sixty day deadline set by fuel protesters ran out.

Earlier that week Gordon Brown, Chancellor of the Exchequer, announced a 3 pence per liter tax cut on low sulphur diesel. As a gesture to farmers and truckers it was no less than a two-fingered one. Low S diesel is virtually unobtainable on these shores!

So, the planned go-slow protest from Jarrow in northeastern England to London set off, as planned.

The raggle-taggle convoy was nowhere near as impressive as organizers had hoped for, but a total 350 vehicles eventually made the journey.

They were, however, denied an opportunity to bring the capital to a standstill. When they trundled towards central London, four days later, they suffered the indignity of being herded onto a specially closed stretch of roadway by police and had to finish the last mile of their journey to Hyde Park's Speakers Corner on foot.

The high point of almost three hours of impromptu speeches was when a few trucks and a haywagon which had somehow evaded the police cordon rolled past, to the rousing cheers of supporters.

As protests went, this was not one of the most memorable. Even campaign leader, David Hanley, threw in the towel by declaring his intention to stand down.

It's a sad reflection on the apathy of the times when, in the space of two short months, public attention can be so quickly diverted from a cause it once espoused wholeheartedly.

Even in the face of social breakdown, this humble band of protesters had been elevated to pioneer status and bystanders enthusiastically cheered from the sidewalks as convoys crawled by displaying eloquent anti-government slogans such as "Tell Gordon Brown 2p off."

It's even more disturbing that one of the factors partly to blame for distracting the fickle attentions of the British public and media from the merits of this noble crusade was the preposterous political posturings in Washington.

Well, that's us Brits for you, always looking on the bright side of life and enjoying the discomfort of those more fortunate than ourselves.

If you would like to contribute articles (1,500 words or less) on renewable or conventional energy, please submit them for consideration to:

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Natural Gas, the Energy Choice of the 21st Century?

A recent promotional flyer from EnergyNorth, a natural gas and propane company located in New Hampshire, alerted us to a study conducted by Washington Policy and Analysis (with the support of the American Gas Foundation), "Fueling the Future: Natural Gas and New Technologies for a Cleaner Twenty-First Century."

EnergyNorth touts the study's prediction that natural gas demand will jump 60% over the next twenty years and that "gas-fired technologies will drive an economy that thrives on cheaper, cleaner energy."

EnergyNorth notes:

> Even more promising, though, is the emergence of new technology that allows businesses—and even homes—to generate their own electricity using natural gas fuel cells and microturbines. The advent of on-site, natural gas powered electric generation has fueled considerable interest by Wall Street in natural gas utilities, many of which are at the same stage now as fledgling "Dot.Coms" were five years ago.

Despite the expected increase in natural gas usage, the study projects only modest gas price increases over the next twenty years. U.S. imports of oil would be reduced (in 2020) by an estimated 20%, 2.6 million barrels per day, the study claims. Also claimed: If the increase in natural gas use in the U.S. in 2020 predicted by the study is reached, carbon dioxide greenhouse gas emissions would be cut by 50% (by 930 million tons annually).

The "Fueling the Future" study is available on-line at www.fuelingthefuture.org.

Compressed Air Car?

BBC News released a report on October 24, 2000 about a small car for city dwellers that is powered by compressed air. The automobile, dubbed the "e.Volution," was to be unveiled at the Auto Africa Expo 2000 in Johannesburg, South Africa.

It is no news, of course, that reciprocating engines can be run on compressed air. That is conventional technology, which powers everything from pneumatic tools to a now popular toy airplane. But the claims for the e.Volution vehicle are extraordinary. It was said by the BBC that the e.Volution could travel up to 200 km on one filling of compressed air for only U.S. $0.30—less than 10% of the cost of fuel for a high-efficiency gasoline engine vehicle. The selling price of the lightweight auto (approximately 700 kg body weight) was to be about U.S. $10,000. Refilling the compressed air tank from its internal electrically driven compressor would take four hours, according to the report. This allows the car to be "refueled" anywhere. However, refilling from central station compressed air tanks could require as little as three minutes.

The engine technology was developed by Guy Negre, a former French Formula One racing car engineer. Two factories will be producing the e.Volution in France, at least one in South Africa, plus plans for factories to be built in Mexico, Spain, and Australia.

Scrapheap Challenge

Soo Seddon

It is, perhaps, the recently discovered fact that the UK is one of the world's worst countries when it comes to effective re-use of waste materials, which prompted TV station Channel 4 to produce their "Hey, recycling can be fun" series "Scrapheap Challenge."

In each program, two teams compete to build something practical out of junk from a breaker's yard. In the last show the challenge set for "The Nerds" from North America and "The Beachboys" from the UK was to build a steam-driven vehicle and race it three times around a 700 yard course laid out at the National Motor Museum at Beaulieu.

The Beachboys immediately dived into the junkyard with relish, grabbing everything they considered remotely practical—the chassis from a jeep, lightweight tractor wheels, and an eighty year old boiler from a naval steam launch. At one third of a ton, the giant boiler weighed more than the three-man team combined. Based, apparently, more on the availability of materials than any overall plan, they decided to build a seriously heavyweight machine with oversized wheels on the back to improve speed.

The Nerds were a much more cautious bunch. They first sketched out some ideas, held a few summit conferences, and then selected what, at first sight, appeared to be a highly improbable combination of raw materials. They took as their base the chassis from a Robin Reliant, a three-wheeled fiberglass monstrosity which, bizarrely, still has quite a cult following in the UK. . .it's little more than a motor tricycle with external cladding.

Referring to their careful diagrams of triple expansion motors and becoming very wrapped up in techno-speak, they eventually agreed to install a chain drive on their machine and abandoned their initial idea of using oversized rear wheels as, on such a lightweight frame, they envisaged it might result in serious steering problems.

At the starting line the two very different machines built up steam and juddered uncertainly onto their first lap. One pit stop was allowed to take on water and a thirty second time penalty would be imposed if it became necessary to get out and push.

Mid-way through the first lap it was neck and neck until the Beachboys' machine snapped its driveshaft on a tight hairpin bend. An emergency welder was called to effect repairs and the Nerds sailed past whistling Dixie.

The American triumph was short-lived, however, when on the second lap they took on too much water in the pits and cooled their engine right down. It was the Brits turn to hurl patriotic abuse when they, newly repaired, sailed passed the becalmed opposition who were desperately attempting to push start their boiler.

Then the Beach Boys suffered a fatal blow yards from the finish line when a sudden loss of oil resulted in major engine failure and the Nerds sailed past the finish line and kept on going. The distant voice of the team captain was heard requesting urgent details on the whereabouts of the brake.

New Energy Times Archive

20TH CENTURY POWER SYSTEM INCOMPATIBLE WITH DIGITAL ECONOMY:

Study Calls for Greater Use of Micropower

Press Release re: World Watch Institute Paper #151

It is a good sign that small, decentralized, off-grid power—"micropower"— is receiving increased attention in the press and in formal reports such as this one by World Watch Institute. It would make us much happier, however, if analysts such as author Seth Dunn mentioned, even if just as a possibility, the advent of off-grid energy sources such as cold fusion or paradigm-busting electromagnetic systems that employ new physics. In due course, we expect to see such additions. For now, the paradigm of "micropower," driven as it is by the critical need for stable sources of electricity for computers and communications networks, is destined to enter the common terminology of this technophilic age.—EFM

Today's giant coal and nuclear power plants are failing to provide the high-quality, reliable electricity needed to power the new digital economy, according to a new report from the Worldwatch Institute, a Washington, DC-based research organization. Power interruptions due to the vulnerability of central power plants and transmission lines cost the U.S. as much as $80 billion annually.

"We're beginning the twenty-first century with a power system that cannot take our economy where it needs to go," said Seth Dunn, author of *Micropower: The Next Electrical Era*. "The kind of highly reliable power needed for today's economy can only be based on a new generation of micropower devices now coming on the market. These allow homes and businesses to produce their own electricity, with far less pollution."

The new micropower technologies, which include fuel cells, microturbines, and solar roofing, are as small as one-millionth the scale of today's coal or nuclear plants—and produce little if any of the air pollution of their larger cousins. Already, the multi-billion-dollar potential of the market for micropower has sent investors scrambling to buy into some of the new companies, sending their share prices soaring earlier this year.

One group of micropower technologies generates electricity by combustion. Reciprocating engines, traditionally fueled by diesel oil and once used largely for backup power, are increasingly fueled by natural gas and run throughout much of the day. Microturbines, advanced gas turbines derived from aerospace jet engines, are just starting to be mass-produced, shipped by the hundreds, and installed in drugstores, restaurants, and other U.S. commercial buildings. Stirling engines, which can run on wood chips and even solar heat, are becoming popular in European homes.

Other micropower systems rely on processes that do not involve combustion. Fuel cells are electrochemical devices that combine hydrogen and oxygen to produce electricity and water. Several hundred fuel cells are already operating worldwide, and will become commercially available for homes in the next one to two years.

Solar cells, or photovoltaics (PV), which use sunlight falling on semiconductor chips to produce electric current, have already entered the residential and commercial building market in nations such as Japan and Germany, and for off-grid use in developing nations. Wind power, the most cost-competitive renewable energy technology, is poised for rapid expansion in rural plains and offshore regions. Small geothermal, microhydro, and biomass systems also hold important roles in the emerging decentralized electricity system.

These small-scale generators have numerous advantages over large-scale power plants. Located close to where they are used, small-scale units can save electricity consumers millions of dollars by avoiding costly new investments in central power plants and distribution systems.

Micropower can also save homeowners and businesses millions of dollars by lowering the threat of power outages and subsequent lost productivity. An electricity grid with many small generators is inherently more stable than a grid served by only a few large plants. Banks, hospitals, restaurants, and post offices have been among the early adopters of micropower systems as a way to reduce their vulnerability to power interruptions. The First National Bank of Omaha, in Omaha, Nebraska, for example, responded to a costly computer system crash in 1997 by hooking its processing center up to two fuel cells that provide 99.9999% reliability.

Use of more efficient combustion-based micropower systems, relying primarily on natural gas, will substantially lower emissions of particulates, sulfur dioxide, nitrogen oxides, and heavy metals. These reductions would range from 50 to 100%, depending on the technology and pollutant.

The use of wind, solar power, and fuel cells fueled by hydrogen can also help reduce global carbon dioxide emissions, one third of which come from electricity generation. In the U.S., widespread adoption of micropower could cut U.S. power plant carbon dioxide emissions in half. In developing nations, small-scale power could lower carbon emissions by 42% relative to large-scale systems.

Micropower will allow developing countries to leapfrog to power sources that are cheaper and cleaner than building more coal or nuclear plants and extending existing transmission lines. Many of these countries lose the equivalent of 20 to 50% of their total power generated through leaks in their transmission and distribution systems. In rural regions, where 1.8 billion people still lack access to electrical services, small-scale systems are already economically superior to the extension of transmission lines-and environmentally preferable to continued reliance on kerosene lanterns and diesel generators. To date, solar PV systems have been installed in more than half a million homes.

Despite micropower's potential benefits, current market rules in most countries favor the incumbent centralized model. Many electric utilities, moreover, perceive micropower systems as an economic threat, and are blocking their deployment by charging onerous connection fees and by paying low prices for power fed into the grid. Failure to reform these rules and practices could result in the construction of another generation of marginally improved large-scale power plants of questionable long-term economic and environmental value.

The extent to which current power markets favor short-sighted solutions is highlighted in the rush to construct some 100,000 megawatts of "merchant plants" worldwide. These large gas-fired power plants, marketed as the answer to power shortages, are designed to make money by selling power in newly deregulated electricity markets when demand and prices are high. But they have raised serious concerns among investors for their financial riskiness, and among grass-roots groups for their negative ecological impacts-as many are located in rural or pristine areas.

The risk of locking in outdated central power plants is even greater in the developing world. Over the next twenty

Micropower continues on page 70

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ECLECTIC OBSERVER

Compiled by Eugene Mallove

New Opening on New Energy in the U.S. Senate

Thanks to the good offices of U.S. Senator Bob Smith (R-NH), a half-day seminar on new energy, cold fusion, and new propulsion physics was held on October 18, 2000 for Senate staff members, under the auspices of the Senate Environment and Public Works Committee. The meeting began at 9:30 a.m. and was followed by a lengthy question and answer period, which extended into the early afternoon. Staff members from the Senate Armed Services Committee also attended.

The initial opening to Senator Smith, which occurred serendipitously, was made by oceanography Professor Ted Loder of the University of New Hampshire. The purpose of the Senate staff briefing was to provide an overview of "outside-the-box" technologies and their critical future impact on environmental trends and the national energy situation.

Speakers gave ten to twenty minute presentations. Participating were: Dr. Ted Loder, Tom Valone, Dr. Paul LaViolette, Dr. Scott Chubb, Dr. Eugene Mallove, Tom Bearden, and Dr. Steven Greer. The responses of the audience questioners indicated a sincere interest by the staffers in the challenging material and its relation to government policies. Present at the meeting, but not participating, was engineer David Hamilton from the U.S. Department of Energy, who has been helpful in promoting openness to new physics technologies.

Aby Mohseni, Chief of Staff for the Environment and Public Works Committee, subsequently reported to the group of speakers that Senator Smith was briefed about the presentations and "was very interested in the subject." We were told by Mr. Mohseni that Senator Smith supports a symposium on "the future problems associated with the current course of energy and fuel production and consumption, and emerging new technologies ('outside-the-box' technologies)." The more extensive symposium for interested Senate staffers and others in the U.S. government, such as from DOE, DOT, NSF, etc., may be held in March 2001. Out of that meeting will come a report which will include a number of recommendations to policy makers and the Senate. An official Senate hearing might then follow in the April to June 2001 time period. *Infinite Energy* will provide information about this as it becomes available.

United States Court of Appeals for the Federal Circuit Rejects Cold Fusion Inventor's Appeal

In an entered judgement and opinion filed November 8, 2000, the United States Court of Appeals for the Federal Circuit (00-1108, Serial No. 07/760,970) found against Dr. Mitchell R. Swartz, *pro se,* of Wellesley Hills, Massachusetts in the appeal of the negative decision on one of his long-standing cold fusion patent applications.

The judgement and opinion rendered by circuit Judge Plager, Senior Circuit Judge Archer, and Circuit Judge "DYK," begins: "Mitchell R. Swartz appeals from the decision of the United States Patent and Trademark Office (PTO) Board of Patent Appeals and Interferences (Board), Appeal No. 94-2920, affirming the examiner's final rejection of claims 25-48 of application Serial No. 07/760,970 for lack of operability or utility under 35 U.S.C. § 101 and lack of enablement under 35 U.S.C. § 112 ¶ 1. We affirm the board's decision."

When asked about this decision, Dr. Swartz told the LENREW scientific conference held in Maryland in November that his patent concerned a technique for measuring electrode hydrogen isotope loading, using acoustic techniques. Those in the cold fusion field who recognize the importance of hydrogen loading as a parameter should be surprised that such a technique would be deemed to have "lack of utility." As informed readers know, the USPTO has, in effect, gone to war against the granting of virtually all cold fusion patents—certainly all those that employ the trigger words "cold fusion." The patent application of Drs. Fleischmann and Pons was killed years ago, after millions of dollars were spent by the State of Utah and ENECO to defend it.

Opposing Dr. Swartz was Albin F. Drost, Acting Solicitor, USPTO, for the Director of the USPTO. Standing with him were John M. Whealan, Acting Deputy Solicitor, Mark Nagumo and Stephen Walsh, Associate Solicitors. Of counsel were Maximilian R. Peterson and Nancy C. Sluter, Associate Solicitors. This is virtually an army of legal expertise arrayed against the *pro se* MIT-trained engineer and Harvard-trained physician Dr. Swartz, who has represented himself articulately and capably in long years of this and other patent battles. Two cold fusion colleagues had submitted "amicus briefs" as "friends of the court": Eugene F. Mallove, Sc.D, amicus curiae, New Energy Research Laboratory and Scott R. Chubb, Ph.D., amicus curiae, Research Physicist, Research Systems, Inc., of Burke, Virginia.

The language of the rejection opinion states, in part: "Here the PTO provided several references showing that results in the area of cold fusion were irreproducible. Thus the PTO provided substantial evidence that those skilled in the art would 'reasonably doubt' the asserted utility and operability of cold fusion." Dr. Swartz had complained that the Board had ignored voluminous evidence for utility; this assertion was rejected as a basis for his defense.

The bottom line is this: It is not possible to imagine proper outcomes in cold fusion patent applications until there is a thorough house cleaning of unethical and incompetent staff at the USPTO. Apparently rogue appeals courts will rubber stamp the USPTO's egregiously faulty actions against cold fusion.

New Energy Times Archive

MIT Nuclear Energy Center Established

The official MIT *Tech Talk* newspaper (October 18, 2000, p. 3) reports the launch of a new engineering center: "In response to the renewed interest in, and opportunities for, nuclear energy worldwide, the MIT Energy Laboratory and Department of Nuclear Engineering have established the Center for Advanced Nuclear Energy Systems (CANES). Some 20 MIT faculty and staff are directly involved in CANES, which will coordinate and expand MIT activities in examining new technology options for future nuclear energy plants and fuel facilities. In addition, it will examine the best approaches to managing and regulating such facilities."

The CANES Director will be Professor Mujid S. Kazimi, Tokyo Electric Power Company (TEPCO) Professor of Nuclear Engineering. The *Tech Talk* article remarks boldly: ". . .the center will also serve as a resource for examining *emerging external ideas for improved or new energy systems."* Our italics. Hmm? Don't all rush to send your latest cold fusion/new energy proposals to Prof. Kazimi, but it was my experience while at MIT from 1989 through 1991 that Prof. Kazimi was willing to learn about progress in cold fusion research. The main focus of CANES work apparently will be conventional fission nuclear power, which is said today to provide about 20% of the world's electricity from about 440 reactors.

Climate Change Twists and Turns

It seems that there will be no internationally agreed terms for nations to reduce emissions of CO_2, at least that was the result on November 25 of the two-week gathering at The Hague by negotiators from 182 countries. U.S and European delegates could not agree on how "credits" for CO_2-absorbing forestation should be balanced against CO_2 emissions. In any event, following the signed non-binding Kyoto protocol of 1997, the U.S. Senate in 1998 passed a resolution (95-0) that the U.S. should not ratify any treaty on climate change that does not have targets for CO_2 emissions reductions by developing nations. The Kyoto protocol specifies that developed countries will by 2012 reduce their CO_2 emissions (in aggregate) by 5% from 1990 levels. The U.S. was targeted for a 7% reduction.

On another front, Andrew Revkin of *The New York Times* (October 26, 2000, A. 18) had earlier leaked details of a new report by the Intergovernmental Panel on Climate Change (IPCC). The assessment was not to have been completed until a meeting in Shanghai, January 2001. Revkin wrote, "A copy of the summary was obtained by *The New York Times* from someone who was eager to have the findings disseminated before the meeting in The Hague." The projections of the IPCC for average global temperatures in 2100 were said to be higher than estimates had been in 1995 (11°F above 1990 in the new report versus 6.3°F above 1990 in the 1995 report). All these projections are made on the basis of computer modeling, whose assumptions and fidelity many have questioned. Revkin quoted the view of Professor Richard S. Lindzen of MIT, one of the most severe critics of climate change models and their doomsday scenarios. Lindzen said that the IPCC report summary consisted of "waffle words designed for one thing, to ensure that the issue remains important enough that it is not to be put on a back burner." But Lindzen did acknowledge an unquantified "human component to the change that's underway."

Quantum Route to Thermodynamic "Perpetual Motion"?

An article in *Science News* (Oct. 7, 2000, Vol. 158, pp.234-235) by Peter Weiss, "Breaking the Law: Can quantum mechanics + thermodynamics = perpetual motion?" alerted us to recent peer-reviewed assessments that "perpetual motion machines of the second kind" might be possible— at least on a microscopic scale. Such postulated thermodynamically "impossible" machines could take energy from a uniform temperature bath and create work without rejecting heat to a lower temperature reservoir. But where quantum phenomena reign, it might be taken advantage of to create, in effect, a real "Maxwell's Demon," one that would not need to consume energy to do its work—quantum phenomena would accomplish that.

The works cited are: Allahverdyan, A.E. and Th.M. Nieuwenhuizen. 2000. "Extraction of work from a single thermal bath in the quantum regime," *Physical Review Letters,* 85(Aug 28), p.179 and Cápek, V. and J. Bok. 1999. "A Thought construction of working perpetuum mobile of the second kind. *Czechoslovak Journal of Physics,* Vol.49, p.1645. Nikulov, Alexey V. of the Russian Academy of Science's Institute of Microelectronics Technology and High Purity Materials (published on physics preprint server:
http://xxx.lanl.gov/abs/physics/9912022

Light Pollution and Saving Energy

The amateur astronomer in me—he's been there since childhood—perked up with a recent mailing from the nonprofit International Dark-Sky Association (www.darksky.org) of Tucson, Arizona. The group's mission is: "Preserving the beauty of our night skies through the promotion of quality outdoor lighting which will conserve energy and improve safety and security—Help Us Save the Stars." Their message is worth pondering:

"Today, city and suburban dwellers have lost sight of most of the universe. The spectacular view of the sky offered our ancestors on dark clear nights no longer exists. The outdoor lighting accompanying urban population growth has overwhelmed the stars with the glow. The problem is not the outdoor lighting—but rather the light wasted upward into the night sky. This 'light pollution' provides no useful lighting, wastes significant amounts of energy, and threatens astronomical research.

City lights of the continental U.S., southern Canada, and northern Mexico.
Courtesy International Dark-Sky Association

New Energy Times Archive

"Light pollution problems can be solved with efficient outdoor lighting, designed to illuminate the ground for safety and utility, without causing a blinding glare or wasting energy.

"A surprising amount of outdoor lighting is poorly designed or not designed at all. Inefficient fixtures, such as the common mercury vapor light, do not efficiently convert electricity to light, and then shine 30% or more of the light upward, where it is wasted. In the United States, the cost of this misdirected light can be conservatively calculated to be more than $1.5 billion annually. U.S. power plants burn nearly 6,000,000 tons of coal to generate this amount of wasted electricity."

Wandering ITER

Physics Today (September 2000, p. 56) reports the latest attempt to find a home for the wandering hot fusion test reactor known as ITER (International Thermonuclear Experimental Reactor). France is officially vying with Japan and Canada for having the device on its soil; Southern France's Cadarache nuclear center is being proposed. Congress pulled the plug on the costly U.S. involvement with ITER ("Money Eater") back in 1998. Hot fusion boosters are happy that at last *any* European country wants it. A location in Canada is seen as a compromise between a site in France or Japan. Another advantage being touted for the Canada site is that radioactive tritium fuel is produced there, so it would not have to be shipped. A home in Canada may also be the best way to lure U.S. money back to the project. The latest auction-like price-tag on ITER is about half the previous $8 billion figure. A site may be chosen by late 2002—construction to begin no sooner than 2003. There may still be time for a commercial cold fusion or other new energy device to kill this homeless and hopeless project.

Magnets and Bioelectrocatalysis

Chemical and Engineering News (December 4, 2000, p. 63) reports that researchers in Israel have developed a means to turn on or off certain biochemical activations by means of magnets. The work is reported in the *Journal of the American Chemical Society* (Vol. 122, (2000) p. 12053). So-called electrocatalytic activation of biological redox reactions can be switched on or off provided that magnetite particles are suitably chemically attached to molecules. Future applications are said to include magnetic control of catalysis in bioreactors or activating biofuel cells. So much for the often-heard charge by critics (such as Professor Robert Park) that magnets cannot affect biological systems.

More Money from "Smart Fusion"

Our junk mail seems to contain at least two or three solicitations for credit cards each week. A recent attempt to sell us on easy money must have had some input from the hot fusion tokamak reactor designers (they should know about easy money). This credit card is called the "Fusion™ smart Visa card." The logo for "The Most Powerful Smart Card" appears to be a stylized skeletal tokamak fusion toroid with the word FUSION™ under it! Apparently it really is "smart," because it has a 32K data chip in it, with which advanced ATM or other machines presumably can interact. But it

promises only up to a $50,000 credit limit. Gee, one would have thought that a hot fusion-inspired credit card would have at least $5,000,000,000 on it.

Why did the chicken cross the road? Schrödinger: Because its wave function collapsed on the other side.

Submitted by George Sassoon

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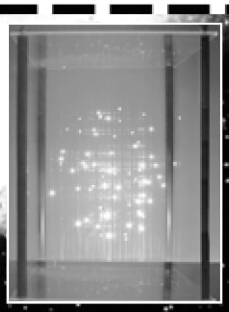

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Upcoming Events

Call for Papers

The Precious Metals Academic Committee of China and the IPMI will co-sponsor a precious metals seminar March 27-29, 2001 in Hong Kong. The theme of the seminar is: Precious Metals and Clean Energy Sources

Papers in the areas of fuel cells, stationary emission control, automotive catalysts, environmental, automotive manufacturing and emission standards, and related refining and precious metals recovery are invited.

If you are interested in submitting a paper, please submit a formal abstract of up to 100 words to: Dr. Martin T. Durney, Johnson Matthey Co., 2001 Nolte Drive, West Deptford, NJ 08066 • Phone: 856-384-7210 • Fax: 856-384-7035 • Email: durnemt@jmusa.com

If you are interested in attending this seminar, please contact IPMI at 850-476-1156 (mail@ipmi.org) to be placed on the mailing list for seminar details as they become available.

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March 12-16, 2001
Seattle, Washington

Quantum Fluids and Solid IV: Cold Fusion Session

March 15, 2001
8:00 a.m.

www.aps.org/meet/MAR01

American Chemical Society's Energy for the 21st Century:

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April 1-5, 2001
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Contact Texaco's M. Rashid Khan
Phone: 713-954-6238
Email: khanmr@texaco.com

199th Meeting of the Electrochemical Society

March 25-30, 2001
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Washington, DC

Contact: T. Zawodzinski
Los Alamos National Lab.

Phone: 505-667-0925
Email: zawod@lanl.gov

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The Plight of the Obscure Innovator in Science

Moti Nissani*

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Abstract

The extent of resistance to original contributions of obscure scientists is controversial. One view holds that such resistance is rare and, hence, that it requires little study or remediation. A second view holds that, although not widespread, such resistance happens often enough to merit study and reform. A third view holds that this resistance is common, that it constitutes the single most formidable block to scientific advances, and that its disturbing regularity calls for a partial restructuring of the modern scientific enterprise. After documenting this crucial controversy and arguing that it cannot be resolved through citation analysis, this note tests one implication of the third view, *i.e.* that even a cursory search of the historical and biographical literature should reveal many cases of bitter struggles for publication and recognition besides the ones which are customarily cited in discussions of this subject. Such a search has been carried out, yielding over fifty names of scientists and scholars who, by all counts, made decisive contributions to their respective fields, but who nonetheless had to struggle to have their results published or recognized. In most instances the original sources from which these cases have been culled are directly quoted, thereby showing that most historians and biographers of science tend to view the struggles they describe as rare and as owing to the peculiar circumstance of the case in question. Most likely, such struggles are traceable to many interdependent sociological, political, and psychological causes. Instead of providing a comprehensive causal analysis, this note highlights one psychological factor which may merit greater attention from social science theorists. Given these diverse roadblocks against obscure innovators, the surprising thing may well be that some unrenowned innovators, in science at least, have escaped the struggle, not that so many haven't. This note urges a systematic historical study to estimate the incidence of resistance. If such a survey shows that obscurity plus originality often lead to temporary or permanent oblivion, the case for structural reforms in science will become immeasurably stronger than it is now.

In his informative and thoughtful paper, Professor Juan Miguel Campanario[1] raises issues which are of the greatest possible importance to scientific progress. In this response, I should like to augment Campanario's review with additional data, interpretations, and reflections.

Three Views on the Extent of Resistance to New Ideas in Science

Everyone agrees that some seminal work has been refused publication, ignored, or scorned by the scientific community. The extent of such rejection is, however, highly controversial. This uncertainty may strike a newcomer to this field as odd, for it can be resolved through systematic historical studies. Nonetheless, such studies are yet to be carried out, and the subject at this writing remains as contentious as it has ever been.

Estimates of rejection frequencies can be best placed along a single continuum. But, for the sake of brevity and convenience, only three distinct headings will be given here. In an effort to document and capture the flavor of this debate, I shall quote at some length the views of representative proponents of each position.

The *minimalist position* states that rejection of new scientific ideas is rare and, hence, that it is of little consequence to scientific progress.

The scholarly ideal of open-mindedness and constructive criticism has been expressed on many occasions. One of the earliest (second century, B.C.) formulations of this ideal can be found in the dialogue between the Buddhist sage Nagasena and the Greek king Menander, a dialogue which set the ground rules for their long subsequent discussions. In response to Nagasena's insistence that they discourse like scholars, the king asks: "How is it that scholars discuss?" To which Nagasena replies:

> 1) When Scholars talk a matter over one with another, then is there a winding up, an unraveling; one or the other is convinced of error, and he then acknowledges his mistake; distinctions are drawn, and contra-distinctions; and yet thereby they are not angered. Thus do scholars, O king, discuss.[2]

Here are the similar views of two more contemporary observers:

> 2) Contrary to a widely held opinion, it is extremely rare for any scientist to publish an epoch-making discovery and have it ignored. As a rule, the knowledge of the discovery spreads rapidly and the scientist is honored promptly and copiously.[3]

The extent of resistance to original contributions of obscure scientists is controversial. [One view] holds that this resistance is common, that it constitutes the single most formidable block to scientific advances, and that its disturbing regularity calls for a partial restructuring of the modern scientific enterprise.

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3) If we ask how Einstein's career fared in the world of professional physicists, we find that whatever resistance he encountered on the road to academic acceptance was due more to politics and anti-Semitism than to physics. . .In the face of [Einstein's] favorable reception. . .it's hard to accept the myth that scientists lack imagination. . .For the past century scientists have been repeatedly chastised with the accusation that they are always turning down unorthodox ideas. One consequence of this criticism has been remarkable. Scientists are able read; they are sensitive to history. Their reaction has been to become exceedingly receptive to new ideas.[4]

The *moderate position* asserts that, although rejection has only plagued a small fraction of obscure innovators, it is frequent enough to deserve serious study and perhaps even a measure of reform:

1) That some resistance occurs, that it has specifiable sources in culture and social interaction, that it may be in some measure inevitable, is not proof. . .that there is more resistance than acceptance in science. . .Nevertheless, some resistance remains, and it is this we seek to understand and thus perhaps to reduce.[5]

2) There are *very few* Citation Classic[R] papers which had problems in being published, but, precisely because of the great impact and importance of the papers involved, their rejection or delayed publication can have pernicious effects. . .It is clear that new ideas and theories, or reinterpretations of previous data, can encounter *some difficulty in being published*.[6]

The *maximalist position* asserts that rejection of obscure innovators is common and, hence, that it poses a serious obstacle to scientific progress.

Upholders of this view argue that the minimalist or moderate views perceive only the spire or tip of this chilling iceberg. The more correct generalization, they say, might well be: Articles which (a) have been written by obscure, not particularly well-off or well-connected, scientists and mathematicians, (b) involve reconceptualizations, innovations, or controversies, and, (c) fail to recruit a well-known champion, often face an uphill struggle for publication and acceptance. Such struggles, maximalists say, constitute the single most important roadblock to scholarly and artistic advances.

Over the years, variations on this view have been expressed by a small, vocal minority, of whose opinions only a sampler will be reproduced here:[7]

1) The myth that merit must inevitably be recognized does not bear the most casual scrutiny. Indeed, the opposite is more likely to be true: mediocrity is sure of its applause and recognition while great merit will, because of its capacity to disturb, generally be overlooked. . .Merit goes unrecognized except by accident. . .there is more. . .great music entirely unknown than there is known. My hunch is that the same applies to painting and poetry too. . .[There are many] whose scientific discoveries when submitted to the Royal Society had been ignored.[8]

2) Very occasionally one thinks. . .something new: and waits a little bleakly for years, in the hope that it will strike a spark of recognition somewhere.[9]

3) The history of [Waterston's] paper suggests that highly speculative investigations, especially by an unknown author, are best brought before the world through some other channel than a scientific society, which naturally hesitates to admit into its printed records matter of uncertain value. Perhaps one may go further, and say that a young author who believes himself capable of great things would usually do well to secure favorable recognition of the scientific world by work whose scope is limited, and whose value is easily judged, before embarking upon higher flights.[10]

4) The history of science abounds in instances of basic papers having been written by comparatively unknown scientists, only to be rejected or neglected for years.[11]

5) But what remains to be said about the quantity and source of the blood which thus passes, is of so novel and unheard-of character that I not only fear injury to myself from the envy of a few, but I tremble lest I have mankind at large for my enemies, so much doth wont and custom, that become as another nature, and doctrine once sown and that hath struck deep root, and respect for antiquity, influence all men.[12]

It would appear, then, that commentators on the plight of obscure innovators can be placed in one of three camps. The first claims that resistance to obscure innovators does not occur often enough to merit the serious attention of sociologists, psychologists, historians, and scientists. The second claims that, although not widespread, the problem occurs often enough to justify description, analysis, and reform. The third believes that resistance is common and that it deserves the closest possible scrutiny and remediation. How does the data culled from Citation Classic[R] bear upon these three views?

Citation Analysis Cannot Resolve This Controversy

In his paper, Prof. Campanario lists sixteen historical episodes of resistance to new ideas, including the well-known cases of Krebs, Mayer, Peters and Ceci, Waterston, and Yallow. Most of Campanario's paper, however, is dedicated to analyzing 316 retrospective commentaries of authors of highly-cited articles. Of these, eighteen (5.7%) encountered some administrative difficulties during the investigation or publication phases. At least some of these problematic papers reported innovative methods or presented new interpretations of previous data. Three of the papers which encountered publication problems are the most frequently cited papers from the journals in which they appeared.

Of these 5.7%, 0.6% encountered some resistance during the research phase, 1.9% encountered some resistance to publication in the journal which eventually published the paper, and 3.2% of the papers were rejected by one journal and published in another.

Campanario's historical and Citation Classic[R] data may be construed as lending support to the moderate view of resistance to change. Only a few historical case histories are described or referenced. And, the great majority of the highly-cited papers in his survey experienced no problems during the research, publication, and recognition phase. Of the small fraction which did, all eventually appeared in print and gained recognition.

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Proponents of the maximalist interpretation might however argue that Campanario's moderate position is a consequence of the paucity of systematic historical studies of this subject and of his methodology, rather than of historical realities. I shall have more to say about history later. At this point, I shall highlight a few methodological shortcomings of citation analysis.

Citations tell us something important about resistance to new ideas in science, but, when used as a key source, they may give rise to incorrect generalizations. To begin with, citation analysis tells us nothing about important papers which were never accepted for publication. If the maximalist position is correct, a careful historical search for such rejected contributions may yield a rich, if heart-rending, harvest. The close misses of Aristarchus, Strato,[13] Waterston, and others, seem to support this view. Also, we know that some highly original contributions have been ignored for decades (*e.g.*, Mendel,[14] Rumford), centuries (*e.g.*, Bernoulli, da Vinci), sometimes even millennia (*e.g.*, Aristarchus' heliocentric theory and Chinese contributions to science and mathematics). Citation data have not been around long enough to detect such instances of delayed recognition.

Many articles are frequently cited for reasons other than creativity. For instance, in chemistry one often cites the source which provides the most practical description of a technical procedure, not the paper in which this procedure was first described. Thus, some highly-cited papers are technical papers which no one, including their authors, would classify as revolutionary. Also, reviews are often highly cited, even when they do not involve original research or insights. All things being equal, contributions written by well-known authors, or by authors from prestigious institutions, are more often cited than contributions of lesser known scholars. Sometimes, citations are a matter of taste, fashion, and personal acquaintance. Moreover, the choice of citing one paper and not another is often determined by accessibility; it makes more sense to cite papers written in English than papers in Hebrew or Urdu, for example. Originality and revolutionary nature, then, are merely one possible reason among many for citing papers. By themselves, high or low citation counts tell us little about quality.

Indeed, one empirical study concludes that "citation counts may not necessarily measure quality." Highly cited articles are simply useful to a large number of scientists. Citations, according to this study, provide quite a limited perspective on quality and "they measure perceived usefulness more than perceived quality."[15] Prof. Campanario cites Eugene Garfield's views on a few occasions in his paper, but the most relevant Garfield's citation, maximalists may argue, is this:

> A highly cited work is one that has been found to be useful by a relatively large number of people, or in a relatively large number of experiments. . .O.H. Lowry's 1951 paper on protein measurements is a classic example. It was cited. . .more than five times as [often] as the second most highly cited work. The only thing the count indicates about this particular piece of Lowry's work was best said by him: "It just happened to be a trifle better or easier or more sensitive than other methods, and of course nearly everyone measures proteins these days. . ." The only responsible claim made for citation counts as an aid in evaluating individuals is that they provide an objective measure of the utility or impact of scientific work. They say nothing about the nature of the work, nothing about the reason for its utility or impact.[16]

A Cursory Survey

The maximalist position leads to two predictions. The first is implied in Francis Bacon's view that, "Time is like a river which has brought down to us things light and puffed up, while those which are weighty and solid have sunk."[17] The second follows from Wilfred Trotter's observation that "the reception of new ideas tends always to be grudging or hostile. . .Apart from the happy few whose work has already great prestige or lies in fields that are being actively expanded at the moment, discoverers of new truths always find their ideas resisted."[18] The first prediction is, in the nature of the case, extremely difficult to test. Besides, unearthing a few episodes of misplaced priority would tell us little about the *extent of resistance*. The second prediction, however, if the maximalist view is nearer the mark than its competitors, should yield a bumper crop in a comparatively short time. In fact, maximalists predict that most original contributions of poorly-connected scientists, mathematicians, technologists, and other innovators, had to overcome publication and recognition hurdles.

Sir Francis Bacon
1561-1626

The data below disclose the outcome of one casual survey. In the interests of brevity, most instances readily accessible elsewhere[19] have been omitted. Whenever possible, the list provides quotations. These often highlight the curious fact that most biographers and historians subscribe to the minimalist position, and that, whenever they encounter a struggle for publication or recognition in science, mathematics, technology, or other fields, they tend to attribute it to the peculiar circumstances of that case history (such as nationality, belligerence, or misfortune), instead of, as maximalists might, to deep-seated psychological, sociological, and political causes. Because this list contains names which are not usually mentioned in this context, and because it is possibly one of the longest of its kind, it should also be of some interest to those wishing to understand the social dimension of resistance to innovation.

1. "A young Swedish student of chemistry named Svante August Arrhenius was the first to suggest, in 1884, that ions are charged atoms. . .His notions, advanced in the thesis he presented for his degree of doctor of philosophy in that year, were so revolutionary that his examiners could scarcely bring themselves to pass him. . .He was awarded the Nobel Prize in chemistry in 1903 for the same thesis that nineteen years earlier had nearly lost him his doctoral degree."[20]

2. Amedeo Avogadro's famous hypothesis of 1811 was "not received by chemists, owing, firstly, to the small number of cases to which it could then be applied, and, secondly, to the fact that several of those cases gave anomalous results not understood until much later. It was not until

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1858, after Avogadro's death, that authoritative attention was called to it by another Italian chemist, Stanislao Cannizzaro. . .This long eclipse of an important law rendered the results of physical chemistry far less profitable than they might have been for nearly half a century."[21]

Amedeo Avogadro
(1776-1856)

3. "Percussion, the science of tapping the chest and other portions of the body and interpreting the resulting sounds to determine the status of the organs beneath, had been evolved by Leopold Auenbrugger. . .His description of his new diagnostic principle in 1761, perhaps because of its long and involved Latin title. . .had largely gone unnoticed by the medical world until translated and popularized in 1808 by Napoleon's personal physician and friend."[22]

4. "A clear understanding of the nature of heat finally came with the. . .realization that the molecules composing a gas are in continual motion, bouncing off one another and off the walls of their container. The first investigator who attempted to explain the properties of gases from this standpoint was the Swiss mathematician Daniel Bernoulli, in 1738, but he was ahead of his times. In the mid-nineteenth century, Maxwell and Boltzmann. . .worked out the mathematics adequately and established the kinetic theory of gases."[23]

5. In 1803, Claude Louis Berthollet published an essay which later became "the basis of modern chemical dynamics," but which had been "unfortunately neglected for many years."[24]

6. "One would think that by the time Boyle published the account of his experiment [on the behavior of a mercury barometer in vacuum] the whole learned world would have accepted the new ideas. But the advance of science was slow in the mid-seventeenth century, in part because of the lack of scientific societies and scientific journals."[25]

7. "When lava cools, the crystals form in alignment with the [Earth's] magnetic field. As long ago as 1906, the French physicist Bernard Brunhes noted that some rocks were magnetized in the direction opposite to Earth's present magnetic field. This finding was largely ignored at the time."[26]

8. Sadi Carnot founded thermodynamics in 1824 but "the pamphlet of Carnot attracted little attention during his lifetime. The principles involved were grasped some twenty years later by. . .Joule."[27]

9. "The entrapment of large particles along the magnetic lines of force had been predicted in 1957 by an. . .amateur scientist, Nicholas Christofilos. . .He had sent his calculations to scientists engaged in such research, but no one had paid much attention to them. (In science, as in other fields, professionals tend to disregard amateurs.) It was only when the professionals independently came up with the same results that Christofilos achieved recognition."[28]

10. "In 1862. . .de Chancourtois found that he could arrange the [chemical] elements in the order of increasing atomic weight in a tabular form, so that elements with similar properties fell in the same vertical column. Two years later. . .Newlands independently arrived at the same arrangement. But both scientists were ignored or ridiculed. Neither could get his suggestions properly published at the time. Many years later, after the importance of the periodic table had become universally recognized, their papers were published at last. Newlands even got a medal."[29]

11. "In spite of occasional dietary victories. . .nineteenth-century biologists refused to believe that a disease could be cured by diet, particularly after Pasteur's germ theory of disease came into its own. In 1896, however. . .Christiaan Eijkman convinced them almost against his own will."[30]

12. "At the start of his career, R.A. Fisher encountered great difficulties publishing papers that are now considered classic. Fisher's 1918 paper was rejected, allowed to languish for more than two years, and was eventually published only thanks to the intervention of Leonard Darwin, an amateur biologist and a nonmathematician with a very famous father and a strong belief in the genius of this obscure schoolteacher whose contributions were largely rejected by the professionals. Later, and in dire financial circumstances, Fisher was offered a job by Pearson, but on the condition that he only publish what Pearson approved. On a few other occasions in his early career, Fisher again experienced publishing difficulties which might have proven insurmountable without Darwin's help."[31]

13. "In 1787, the American inventor John Fitch built a steamboat that worked, but it failed as a financial venture, and Fitch died unknown and unappreciated. Robert Fulton, a more able promoter than Fitch, launched his steamship. . .in 1807 with so much more fanfare and support that he came to be considered the inventor of the steamship."[32]

14. Alexander Fleming "dutifully published his [penicillin] results in 1929, but no one paid much attention at the time. Ten years later. . .Florey and. . .Chain became intrigued by the almost forgotten discovery and set out to try to isolate the antibacterial substance."[33]

15. The mathematician Frege "published his first work in 1879. . .but, in spite of the epoch-making nature of his discoveries, he remained wholly without recognition until. . .1903."[34]

16. Among early hybridizers, Mendel singled out especially Gaertner and Koelreuter. Gaertner failed to achieve recognition in his lifetime, even though he had money and was the "son of a world-famous botanist. In 1849, despairing of getting a willing publisher for his great book, he paid for the printing himself." Koelreuter's "book never achieved a wide circulation. . .Sad to relate, the records of all these experiments were passed over by almost all his contemporaries and forgotten. . .Koelreuter. . .was frustrated and bitter to the end of his days."[35]

17. "In 1876, the American physicist Josiah Willard Gibbs worked out the theory of chemical thermodynamics in such detail that this branch of science was brought from virtual nonexistence to complete maturity at one stroke. The long paper in which Gibbs described his reasoning was far above the heads of others in America and was published. . .only after considerable hesitation. Even afterward, [the subject

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was kept] under a bushel basket until Ostwald discovered the work in 1883, translated the paper into German, and proclaimed the importance of Gibbs to the world."[36]

18. In the night before his death in a duel, Everist Galois, "one of the most original mathematicians of all times. . .revised a paper to the Academie des Sciences (which had previously rejected it as unintelligible). . .It was only after fifteen years, that, with admiration, scientists became aware of the memoir which the Academy had rejected. It signifies a total transformation of higher algebra, projecting a full light on what had been only glimpsed thus far by the greatest mathematicians."[37]

19. Robert Hutchins Goddard's accomplishments with rockets "got almost no recognition, except from his outraged neighbors."[38]

20. William Harvey "was subjected to derision and abuse and his practice suffered badly. Only after a struggle of over twenty years did the circulation of the blood become generally accepted."[39]

21. "In 1905 a Danish astronomer, Ejnar Hertzsprung, reasoned that a cool star must have a dim surface, but if it had a very large surface, the dimness of each bit would add up to a great total brightness. In other words, a bright star that was cool and red had to be a very large star indeed in order to be bright. Hertzsprung published this idea in a journal of photography and astronomers didn't notice it. Then, in 1914, the American astronomer Henry Norris Russell had the same idea independently, and this time the idea stuck and both astronomers are usually given credit."[40]

22. Karl Janski, the founder of radio astronomy, "was largely unappreciated in his lifetime."[41]

23. "Some time in the 1760s a young medical student, Edward Jenner, was consulted by a Gloucester dairymaid who felt out of sorts. Jenner thought that she might be suffering from smallpox, but she promptly replied: 'I cannot take the smallpox because I have had the cow-pox.' After nearly twenty years of struggle against the skepticism and indifference of the medical profession, Jenner succeeded in proving the popular belief that people who had once caught the cow-pox were immune against smallpox."[42]

24. Joule, "who was a brewer by profession and also lacked academic credentials, had difficulty getting his meticulous work published."[43] Joule's "superb exposition of 1847 had been given in the form of a popular lecture in a church reading-room! This great scientific pronouncement, after rejection by several journals, appeared in a Manchester weekly paper."[44]

25. After describing the effects of lead poisoning in a 1786 letter, Benjamin Franklin concludes: "This, my dear friend, is all I can at present recollect on the subject. You will see by it, that the opinion of this mischievous effect from lead is at least above sixty years old; and you will observe with concern how long a useful truth may be known and exist, before it is generally receiv'd and practis'd on."[45]

26. "Trying his treatments on scurvy-ridden sailors, [by 1747 James Lind] found that oranges and lemons brought about improvement most quickly. Captain Cook, on a voyage of exploration across the Pacific from 1772 to 1775, kept his crew scurvy-free by enforcing the regular eating of sauerkraut. Nevertheless it was not until 1795 that the brass hats of the British navy were sufficiently impressed by Lind's experiments (and by the fact that a scurvy-ridden flotilla could lose a naval engagement with scarcely a fight) to order daily rations of lime juice for British sailors."[46]

27. W.H. Lewis not only discovered pinocytosis in 1931, "but was thoroughly aware of its significance. . .But for twenty years or more pinocytosis remained 'unhonoured and unsung'. . .Nowadays references to the phenomenon are constantly encountered in the literature."[47]

28. "McMunn discovered cytochrome in 1886, but it meant little and was ignored until Keilin rediscovered it thirty-eight years later and was able to interpret it."[48]

29. It was "scientific skepticism which brushed aside all the instances of hypnotic phenomena. . .and which—even in the face of the systematic demonstrations of hypnosis by Mesmer and his successors—denied for another century after Mesmer's first appearance the reality of hypnotic phenomena. When the medical profession ignored such palpable facts as the painless amputation of human limbs, performed before their own eyes in hundreds of successive cases, they acted in the spirit of skepticism, convinced that they were defending science against imposture."[49]

30. "The ancients even witnessed falls of meteorites to the earth and found some to be lumps of iron. . .During the eighteenth century. . .science made a backward step in this respect. . .Farmers who came to the Academie Francaise with samples of meteorites were politely, but impatiently, shown the door."[50] "The French Academy of Science stubbornly denied the evidence for the fall of meteorites, which seemed massively obvious to everybody else. Their opposition to the superstitious beliefs which a popular tradition attached to such heavenly intervention blinded them to the facts in question. . .Many public museums threw away whatever they possessed of these precious meteorites: it happened in Germany, Denmark, Switzerland, Italy and Austria."[51] "When, in 1807, two Connecticut scholars. . .reported having witnessed a fall, President Thomas Jefferson said that he would sooner believe that two Yankee professors would lie than that stones would fall from heaven."[52]

31. "In 1920, a Yugoslavian physicist, Milutin Milankovich, suggested a cycle [of Earth's average temperature] that was 40,000 years in length. . .The earth would, by this theory, be particularly susceptible to glaciation in the time of the 'Great Winter'. . . Milankovich's suggestion did not meet with much favor when it was advanced; but in 1976, the problem was tackled."[53]

32. Despite his position at Yale University, and despite his former apprenticeship with the well-known social psychologist Solomon Asch, Stanley Milgram's "first submission. . .was rejected, as was his subsequent request."[54]

33. "Blowfly attack in sheep is a major problem in Australia. . .Several teams of scientists. . .devoted full-time research to the problem, but after many years were able to contribute little towards a means of prevention. A sheep farmer named Mules. . .[developed a surgical procedure which] has proved to be a most valuable means of preventing the disease. Incidentally, there was considerable resist-

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ance even among scientists to the acceptance of Mules' discovery. . .Probably the underlying reasons for the resistance were that the discovery was made by an outsider and that it involved a new principle, namely, the application of surgery to the prevention of a specific disease. It provided an interesting present-day example of the reception *usually* accorded to innovators though in this instance it did not amount to persecution.[55] (italics added)

34. Charles Sanders Peirce, "probably the most versatile philosophic intellect of the nineteenth century in America," was ignored and unappreciated in his lifetime. He died "a frustrated, isolated man. . .without a publisher, with scarcely a disciple, unknown to the public at large. . .The spectacle of America's greatest philosophic thinker of the nineteenth century, writing in the toils and travail of obscurity and neglect, is not a felicitous one for his fellow Americans to contemplate. . .The essence of his thought was originality in every subject he taught. . .It seems a great pity that as original a man as he is. . .should be starved out of a career."[56]

35. William Prout's 1815 paper called attention "to the closeness with which the atomic weights of the elements, expressed in terms of relation to hydrogen, approximated to whole numbers." Prout's paper "had a somewhat similar history" to that of Avogadro's.[57]

36. "In 1676. . .Roemer did succeed in timing the speed of light—on an astronomical distance scale. . .His estimate was "high enough to evoke the disbelief of his contemporaries. Roemer's results were, however, confirmed a half-century later."[58]

37. "Roentgen's first announcement of his discovery of X-rays" was greeted with ridicule.[59]

38. "In science, as elsewhere, progress must struggle against inertia. The caloric theory had been accepted for a very long time, and Rumford's hypothesis [that heat is a form of motion] was so revolutionary that its acceptance was very slow in coming."[60] Rumford's and Davy's experiments on the nature of heat "should have been conclusive; but the caloric theory, though obviously wrong, persisted to the middle of the nineteenth century."[61]

39. "In 1839, Josef Skoda published "a treatise on percussion and auscultation which, at first ignored and even ridiculed, later became famous."[62]

40. Herbert Spencer, D'Arcy Thompson, and Thorstein Veblen refused honors because they arrived too late.[63]

41. John Speke, who had "actually got to the source of the White Nile in Uganda. . .had to wait twenty years or more before his discovery was acknowledged."[64]

42. "When Henry Thoreau died in 1862 at the age of forty-four he was virtually unknown to the reading public. . .It was well over half a century after his death before Thoreau's very special qualities and the real meaning of his work began to be appreciated."[65]

43. Besides discovering Pluto, Clyde Tombaugh was among the first to discover the clustering of galaxies. "Erik Holmberg and Anders Reiz independently demonstrated that the Local Group, which includes our galaxy, is part of a much larger swarm of galaxies." The work of all three was ignored. Tombaugh urged Hubble to examine his data, but in a scene reminiscent of the astronomers who refused to look through Galileo's telescope, Hubble declined: "Hubble remarked merely that the plates he had seen, taken in small areas but over selected regions spread out over the sky, showed a fairly uniform distribution. Tombaugh then offered to show him his records, in which he had kept careful count of the number of galaxies on each plate. Hubble was apparently not interested in seeing those either."[66]

44. "The world of science for many years ignored Tswett's [1906] discovery [of chromatography], possibly because he was only a botanist and only a Russian, while the leaders. . .at the time were German biochemists. But in 1931, a German biochemist, Richard Willstatter rediscovered the process, whereupon it came into general use."[67]

45. In 1973, Nobel laureate Harold Urey "published a little paper in *Nature* arguing that several of the extinction events of the past 40 to 50 million years had been caused by impacts of large comets. Unlike his predecessors, Urey came armed with evidence. . . Once again silence reigned. Urey's paper. . .caused not a ripple despite his prominence and authority in science."[68]

46. "For an American geology student, even as late as the early 1960s, Wegener was anathema. He was not assigned to the lunatic fringe—his other contributions spared him that—but he was close. He was mentioned in textbooks and classes only for comic relief and to provide a bit of the history of geology. Papers supporting drifting continents rarely got through peer review—and often were rejected by editors without review."[69] According to one early supporter of Wegener's hypothesis of continental drift, "deep conservatism is characteristic of the whole history of geology."[70]

47. In 1903 "the Wright brothers got off the ground in a propeller-driven glider and stayed in the air for 59 seconds, flying 852 feet. It was the first airplane flight in history, and it went almost completely unnoticed by the world at large."[71]

Conclusion

Of the three views on the obscure scientist's struggle for recognition, the historical evidence collated here would seem to tentatively support the maximalist view. Why then is this view only upheld by a small minority?

To begin with, the evidence in favor of the maximalist view is far from being conclusive. Also, this view demands a shift in our conceptions of science and scientists. We have all been raised "with the stereotype of the scientist as 'the open-minded' man,"[72] and we tend to view any allegation or evidence to the contrary with incredulity.

Another obstacle for open-minded evaluation of the maximalist view is its perceived implausibility. Historians of science and students of conceptual shift know that it is extremely difficult to abandon untenable beliefs in the absence of a reasonable alternative.[73] For instance, Semmelweis was laughed at in part because he was unable to *explain* the observed effect of soiled hands on childbed fever. So, to be believed, widespread resistance to obscure innovators must be documented *and* explained. Over the centuries, many worthwhile explanations have been put forward.[74] These theoretical treatments do indeed lend greater plausibility to the maximalist position. They place, in particular,

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an emphatic question mark on the textbook dogma of impeccable scientific rationality and fairness. Although they have failed to do so yet, one day they may also spur a systematic historical study, a study which will most likely resolve the controversy regarding the plight of obscure innovators.

My own view, as I have argued at some length elsewhere,[75] is eclectic. That is, I believe that resistance can be traced to a multiplicity of interconnected causes. Instead of attempting here a comprehensive survey of all the factors which make up this complex equation, I shall briefly touch upon one factor which merits, in my view, closer attention than it has so far received.

This understated contributory factor can be best approached through the work of Peter Marris.[76] Based on his studies of bereavement, Marris argues that the process of abandoning a conviction is similar to the working out of grief. According to this view, in many seemingly diverse situations, change requires overcoming an impulse to restore the past. "The impulse to defend the predictability of life is a fundamental and universal principle of human psychology." Human beings possess "a deep-rooted and insistent need for continuity." Once we understand the anxieties associated with the loss of cherished convictions, the tenacity of clinging to old beliefs can be understood. We often argue, for instance, about the need for social change, and we tend to explain conservatism as ignorance, cowardice, or protection of privilege. This is true in some cases, but our resistance to change is often traceable to a universal conservative impulse which is more pervasive and profound than simple prejudice or class interest. One extreme manifestation of this impulse is the frequent failure of people undergoing psychotherapy to restructure their mistaken, dysfunctional, and painful viewpoints and habits.

. . .our resistance to change is often traceable to a universal conservative impulse which is more pervasive and profound than simple prejudice or class interest.

Throughout history, this human tendency has been noted by countless observers of the human condition. "The mind," says Wilfred Trotter, "likes a strange idea as little as the body likes a strange protein and resists it with similar energy. It would not perhaps be too fanciful to say that a new idea is the most quickly acting antigen known to science. If we watch ourselves honestly we shall often find that we have begun to argue against a new idea even before it has been completely stated."[77] "The itch to suffocate the infant ideas burns in all of us," says Walshe.[78] Indeed, the philosopher Schiller believes that this inertia "deserves to rank among the fundamental 'laws' of nature."[79]

There is overwhelming experimental evidence in favor of the assertion that "there is in all of us a psychological tendency to resist new ideas which come from without just as there is a psychological resistance to really radical innovations in behavior or dress."[80] For instance, preconceptions of science students are remarkably resistant to change.[81] "Students. . .do not rush to embrace new viewpoints. Rather, they cling to ideas that form part of their world view even when confronted by information that does not coincide with this view."[82] When scientists, likewise, serve as unwitting subjects in a psychological experiment on belief revision, and when their spuriously-acquired false belief is sharply contradicted by direct evidence, they are disinclined to doubt this belief on the verbal level and are even less likely to let go of it in practice.[83]

Given the documented presence[84] of conceptual conservatism in the laboratory, classroom, clinical psychology, everyday experiences, political history,[85] business world,[86] preliterate societies,[87] and cults;[88] given, moreover, the probable involvement of many other psychological factors; given, above all, the decisive importance of institutional, sociological, and political factors; the frequent struggles of obscure innovators are to be expected. Perhaps we have been directing our astonishment at the wrong target all along. The surprising observation, according to this view, is not that so many obscure innovators suffered, but that some achieved renown with scarcely a struggle. And it is on this score, perhaps, that the natural sciences can boast a prouder record than other scholarly and artistic areas.

Bernard Barber commented that "one of the interesting aspects of the social process of discovery—the resistance on the part of scientists themselves to scientific discovery. . . has been largely neglected as a subject for systematic investigation."[89] A third of a century later, the subject has not yet received the methodical attention it so richly deserves. Only a time-consuming comprehensive historical survey will help to unravel the extent, nature, causes, consequences, and cures of this resistance. The issues raised by Prof. Campanario and others can only be resolved through such an ambitious research program. In the meantime, this note raises the possibility that prolonged struggles for acceptance by the obscure innovator may be far more common than a casual reading of the literature might suggest. This note goes on to show that their high incidence be readily explained by—and predicted from—psychological, political, and sociological theory.

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Lindemann has described. The video tape gives enough general and specific information that a detailed design can confidently be built by any competent tinkerer with a working knowledge of electronics and electric power circuitry. He does not give a specific design, nor does he demonstrate one or present its measured performance. One can easily exclaim, "Where's the beef?" Yet the elaborate argument given in detail with plenty of justification for each and every point makes it clear that the concepts behind the interrelated devices may in all likelihood be put to practical use. In watching this tape, I got a cool drink of water on this topic which had always left me wanting and thirsty in the past. To use other colorful descriptions, though he is not handing you an energy solution on a silver platter, you can sink your teeth into this one. ***

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Pull the Plug on the Department of Energy

John R. Kasich*

It's long past time for Department of Energy officials to drop their "dog ate my homework" alibis and admit that the critics were right all along: The department cannot be reinvented; it should be abolished. The DOE suffers from a disjointed and incompatible set of missions—from nuclear weapons security to civilian energy research to publicizing the benefits of home insulation.

And it doesn't seem to do any of them well. A glance at the pump price at your local filling station reveals the department's lack of success in assuring "affordable and dependable supplies of energy for our nation," as its mission statement boasts.

The Department of Energy was created in the 1970s to solve the energy crisis. But its activities have done little or nothing to further that goal.

A prime example of the DOE's wastefulness is the old Synfuels Corp., which spent billions of taxpayer dollars and delivered no fuel at all. Since the demise of Synfuels, the department has spent additional billions of dollars on other energy research.

This activity has not been cost-effective, particularly in applied research and development, which private industry has ample incentive to undertake on its own. Indeed, some of the DOE's energy research activities are simply thinly veiled corporate welfare.

Since 1995, I have supported numerous bills in Congress to abolish the Department of Energy. The House version of the fiscal year 1996 budget would have abolished the department, transferred its nuclear weapons function to the Department of Defense, and consolidated its civilian energy programs. The Clinton administration steadfastly opposed all these proposals, characterizing them as "reckless."

The General Accounting Office has been a persistent critic of the DOE. Five years ago, the agency reported: "DOE suffers from significant management problems, ranging from poor environmental management of the nuclear weapons complex to major internal inefficiencies involving poor contractor oversight, inadequate information systems, and workforce weaknesses." Time has not softened the GAO's criticism. In the last year, the agency criticized "longstanding weaknesses" at the DOE, including the management structure; failures in managing environmental cleanup projects; and an inability to manage its disparate missions. Finally, the GAO noted that "for decades, DOE has failed to respond to reports by GAO, external experts, and its own consultants that highlight these weaknesses."

Compounding the lack of focus is a bloated DOE bureaucracy. With almost 16,000 employees and more than 100,000 contract workers, the DOE receives a budget of nearly $17 billion. Last year's report of the President's Foreign Intelligence Advisory Board on DOE security, chaired by a respected former senator, Warren Rudman of New Hampshire, repeatedly faulted the Energy Department's bureaucratic "culture" for allowing administrative convenience to take precedence over proper security procedures: "The panel has found that DOE and the weapons laboratories have a deeply rooted culture of low regard for and, at times, hostility to security issues, which has continually frustrated the efforts of its internal and external critics."

Again, the warnings went unheeded. And now, in the wake of the computer hard-drive fiasco, the GAO has just completed a report identifying more than seventy-five incidents of foreign spies targeting U.S. nuclear scientists traveling abroad. The report faults the Energy Department for underestimating the intelligence risks posed by these trips. The GAO also said the department's counterintelligence database, which is supposed to keep records of the foreign trips, had not been properly maintained.

Enough is enough. The department's legion of critics has been warning for years that, given the DOE's incompatible missions and bureaucratic bloat, security lapses were not only possible but nearly inevitable. Our national security is too precious to be placed in jeopardy by the Department of Energy. The critics in and out of Congress were right five years ago, and the situation has only gotten worse. It's time to abolish this bureaucratic monstrosity.

This syndicated article appeared in newspapers across the country on July 12, 2000. It is reprinted with permission of Representative Kasich's office.

*Representative John R. Kasich (Republican-Ohio) is currently serving his ninth term in the House of Representatives. He is chairman of the House Budget Committee.

Editor's Note: In 2001, former Congressman Kasich is pursuing new career directions.

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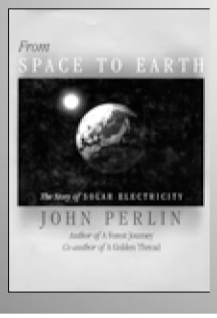
Book Reviews

Practical Photovoltaics: Electricity from Solar Cells

by Richard J. Komp
ISBN 1-55750-951-4
$32.95 Hardcover, 331 pp.
Naval Institute Press, 1999



From Space to Earth: The Story of Solar Electricity

by John Perlin
ISBN 0-937948-14-4
$32.00 Hardcover, 224 pp.
Aatec Publications, 1999
Review by Mike Carrell

These books complement one another nicely. Komp's book dates from 1995 and covers a bit of the history, from the discovery of the photoelectric effect in selenium to the contemporary silicon-based solar cell. He includes a cookbook discussion of how solar cells work, and how to build working solar panels from the basic wafers.

Perlin's book dates from 1999 and relates in a fluent and interesting style the history of solar electricity from the early discoveries through the stages of application and deployment. He unfolds the human dramas as advocates fought organizational inertia to get niche applications funded and demonstrated, starting an expanding spiral of lowered costs and wider applications.

The photovoltaic (PV) properties of selenium were discovered in the last century, but the efficiency is so low (< 1%) that solar power applications are unfeasible. The heating effect of solar energy trapped by glass was known from Roman times and "solar" heating features were incorporated in building design. Solar power plants based on large arrays of reflectors are technically feasible; some are still in operation. These are at a disadvantage because of large capital and maintenance costs, except in special circumstances. Efficient capture of the sun's energy requires some kind of tracking system, conversion to electricity is inefficient, and significant capital expense is required to transmit generated electricity from the convenient collection point to utilization points.

The semiconductor industry laid the foundation for the modern silicon-based PV industry. PV is less demanding of chemical purity and crystalline perfection than transistors, so the early PV industry could use scrap from semiconductor wafer plants as a starting point. Efficiencies of 5 to 6% were realized, enough to justify use in niche applications.

Among the early niche applications was power for navigation warning lights on buoys. A small battery could be charged by day and power a strobe light by night. The lights were mandatory; maintenance costs for engine-generator or replaceable batteries was so high that even expensive PV-battery systems had immediate payback.

Initial designs for active satellites assumed small nuclear power plants, but concerns for environmental consequences of launch failures relegated nuclear power sources to deep space probes where the sunlight is weak. The fleets of active satellites now rely on PV solar collectors, supporting an industry where the cost of the cells is trivial in a cost-benefit analysis.

As the basic reliability of PV cells was demonstrated, other niche markets emerged. Oil wells require cathodic protection against corrosion for buried pipework. Small solar arrays can generate enough voltage and current to protect well components at locations far from power lines. Signaling power is provided along railroad tracks.

Design changes in microwave repeaters and the introduction of semiconductors drastically lowered power costs, so repeaters could be powered by PV-battery systems. This enhanced the use of microwave repeaters long distance telephone service, contributing to the growth of Sprint and MCI. Long distance service could be brought to remote communities. Motorist emergency call stations powered by PV-battery systems are provided on many expressways.

In Africa, water is scarce and people walk miles to collect jugfuls. Father Verspieren initiated Mali Aqua Viva, and in 1975 initial wells were dug. Human-powered pumps drew insufficient water, diesel pumps required constant maintenance support. Dominique Compana designed the first PV-powered pumping system, installed in Corsica. Verspieren visited the system and decided to try a PV-pumping system in Mali Aqua Viva's network of wells. He gathered initial funds from family connections and public appeals in Europe. The system was highly successful, providing ample water but also electricity for lighting and communication. Mali Aqua Viva now has many PV powered pumping systems and is one of the largest commercial customers for solar cells in the world. Similar initiatives have sprung up in many locations.

An important element in the success of these systems is local participation to generate a sense of ownership and responsibility for the system, and economical use of the provided power. Financing plans to defer the initial costs ease the acceptance of PV-battery systems into poor areas, while demonstrating to banks that poor people would honor debt for systems that provide desired services.

The synergy of low powered communication devices and improvements and the cost-performance tradeoff in solar cells is changing the face of the developing world. The model of large central power plants and distribution systems is bypassed. Many needs at the village level—water, irrigation, lighting, communications—are met by PV-battery systems.

Improvements in PV cells are leading to incorporation in building projects where the cells can become part of the skin of a building or the roof of a house. Efficient electronic inverters enable sale of surplus power to existing utility grids and utilization of the grids for power at night or times of low solar input. In this way, the model of large central PV collectors is being supplemented or bypassed in an incremental way, driven by micro-economics and the ingenuity of individuals responding to market incentives.

Perlin's book fills the preceding outline in with many details and anecdotes, making an interesting afternoon's reading.

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Commentary

Perlin well illustrates how a new technology can grow from an effect to an industry through the operation of market forces. Government applications gave a critical boost—not as a subsidy, but because the government provides essential services which are not driven by short-term market considerations, such as satellites, navigation buoys, and the like. In these, PV systems were the most cost-effective solution.

The development path follows the technology. The ingenuity and enterprise of individuals is essential. Institutions usually get in the way; yet individuals within institutions made critical differences. Cold Fusion Technology, *IE*'s parent company, is working with researchers to produce small scale, replicable cold fusion cells in the expectation of initiating an applications spiral like that chronicled in Perlin's book. We are roughly at the point of discovery of the photoelectric effect in selenium; it is there, small, not well understood, but of a character that warrants long-term investment.

The hope for wind or solar farms to replace the heavy duty transportation and manufacturing energy needs seems yet remote. The costs of energy storage and transmission are not affordable over much of the world. Cold fusion experiments indicate potentially high energy densities and low cost consumables, but significant work remains to produce portable, reliable energy systems. BlackLight Power has indicated that batteries based on Mills' hydrino hyrides can achieve energy densities far, far beyond anything now known. If successful, these could transfer energy from solar or wind farms to power grids or automobiles.

There are many technologies developing. How they develop will be interesting to watch. Perlin's lucid book is perhaps a sketch of the shape of things to come.

Discovery of the Cold Fusion Phenomenon: Development of Solid State-Nuclear Physics and the Energy Crisis in the 21st Century

by Hideo Kozima
ISBN 4-87186-044-2
$60.00 Paperback, 370 pp.
Ohotake Shuppan, 1998
Review by Eugene Mallove

Professor of Physics Hideo Kozima of Shizuoka University in Japan can rightfully lay claim to having written the first "textbook" on cold fusion, so comprehensive is its content. Written in English, this is evidently a labor of love by one who is both a thoughtful observer of the history of science and a physicist who has developed his own theoretical understanding of diverse cold fusion phenomena. He calls this the "Trapped Catalyzed Neutron Fusion" (TCNF) model, for which he has exhibited considerable passion at international cold fusion conferences and in numerous technical publications.

The book blends some of the history of the cold fusion controversy with extensive coverage of the experimental evidence for cold fusion phenomena (including effects in ordinary hydrogen systems), how the author's TCNF model can explain these, and a perceptive review of many other episodes in the history of physics that seem to have relevance to cold fusion. He posits the existence of catalytic thermal energy neutrons, for which he says: "If we assume an existence of thermal neutrons in a material, almost all the riddles of the cold fusion phenomenon disappear."

Kozima's book is a mixture of highly technical content with very accessible historical and philosophical discussions that illuminate the process of science in the cold fusion controversy. Kozima's work is not a thorough history of various phases of the cold fusion saga, as are the positive cold fusion books, my own *Fire from Ice* (1991) and Beaudette's *Excess Heat* (2000), which were clearly aimed at more general audiences, though this book too in large measure is certainly accessible to a wide audience. As mentioned, this is more like a textbook of the subject, which offers a potpourri of diverse and expanding areas, such as heavy element transmutation. It is a first work by a pioneer in what are certain to be many more such texts. We understand that other cold fusion scientists may be contemplating or preparing such books. There is a great need for such works to cover the topics of cold fusion calorimetry, nuclear instrumentation, materials science, and theory.

It is noteworthy that Kozima's book is the second excellent cold fusion book from Japan (in English by Japanese cold fusion scientists), the other being *Nuclear Transmutation: The Reality of Cold Fusion*, by Dr. Tadahiko Mizuno (Infinite Energy Press, 1999). While the latter was expertly translated by Jed Rothwell from its original Japanese, the slight difficulty with the Kozima book is its "Japanese English" unevenness, which was not particularly bothersome to this reviewer, and occasionally enjoyable. One very charming example (p. 296): "This Huizenga's conclusion is one deduced by a poor brain only working on an extension line from muon catalyzed nuclear fusion where occurs surely d - d direct fusion reaction." It should be noted that Kozima's book first appeared in hardcover edition in Japanese in March 1997.

Some of the extras offered by Prof. Kozima's work are independent name and subject indices, thirty-seven pages of cited references, generous presentation of pertinent graphical data, and scientist contributed essays by Makoto Okamoto, Akito Takahashi, Francesco Celani, Benjamin Filimonov, and Peter Glück. All in all, an impressive book that deserves to be read widely.

Discovery of the Cold Fusion Phenomenon: Development of Solid State-Nuclear Physics and the Energy Crisis in the 21st Century

by Hideo Kozima

Paperback, 1998, 370 pp.
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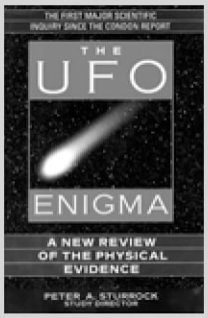

The UFO Enigma: A New Review of the Physical Evidence

by Peter A. Sturrock
ISBN 0-446-52565-0
$23.95 Hardcover, 401 pp.
Warner Books, 1999
Review by John O'M. Bockris

The modern phase of the UFO phenomenon began in 1947. At the very beginning, it was treated seriously and even studied by scientists, as a curiosity. However, as the scope and extent of the phenomenon grew, the official attitude—at least in the U.S.—changed, and by 1969, largely a result of the U.S. government's Condon Report, it was, so to speak, put to bed. Most of the observations were described as illusional or misinterpretations, some of them due to alleged malfunction of equipment (however, even in the Condon Report, a small number of observations were declared to have no rational explanation).

Hereafter, the scientific community closed ranks, and the subject passed into a state of ridicule. Those who "believed" in UFOs were subject to rejection and their sanity questioned. Nevertheless, a number of enthusiastic UFO groups were formed and many books and reports have continued to be published on the subject up to the present time.

Since 1969, reports of unexplained objects visible in the skies have continued worldwide, and the number of these made by apparently reliable observers seems to have increased. The phenomena have been extended to include claimed abductions of individuals into the alleged space vehicles to be later returned unharmed, a few of them showing signs of minor physical injuries.

In view of the prevailing attitude of ridicule, particularly among scientists in universities, it is a brave professor who is willing to put his reputation on the line and come out with an utterly serious book which contains evidence from which it is very difficult to avoid the conclusion that vehicles from some unknown source are frequently seen in our atmosphere.

One of the positive political phenomena in the U.S. is that research projects can be supported not only by the government but also by wealthy individuals, and there's a case in point here because it was Laurance Rockefeller who declared himself to be interested in UFO phenomena and who funded the investigation which led to the writing of the present volume.

The main thing about the credibility of such a volume, of course, is the reputation of its author and the colleagues he chose to work on the project.

Prof. Peter Sturrock is the author concerned. It is he who was invited by Laurance Rockefeller to organize the workshop, the proceedings of which form most of the book's material. It would be difficult to find a scientist who could more effectively represent scientific solidity and rationality in this area than Peter Sturrock. He studied at the Cavendish Laboratory of the University of Cambridge and the Ecole Normale Superieure in Paris. In the U.S. he has been a member of the faculty at Stanford University for many years, as professor of space science and astrophysics. Accordingly, the second half of his career has been devoted to space research, plasma physics, and solar physics. He has chaired a committee of the National Academy of Sciences which reviewed solar physics.

I give these details to evidence the basic strength of the book, which in the degree of its scientific integrity is the highest. As to the scientists whom Sturrock recruited to work with him on the project, these have all been career men in universities or research institutes having a scientific integrity commensurate with that of the chairman.

Thus, after the publication of so many books on UFOs, what was needed was a book written in which the stress was entirely on a high quality in the scientific observations, each piece of data mentioned having been looked at with a very critical eye by those on the committee. What is reported, then, consists of 99%-probable type of truth, which is that good scientific regimen.

The book is in five parts. The first concerns the history of UFOs from 1947 to 1969. The Condon Report is given a detailed and fair treatment, but it is brought out that there are parts of the final report which appear to have differed from what the original contributors had wanted to say in their respective sections.

In a chapter on the aftermath of the Condon Report, the author brings out the great difficulties, in the late 1960s, of being able to publish any serious analysis of the UFO phenomenon. The interest among scientists had dried up, since the subject had been declared no threat to national security. The data sank to the level of "stories."

The second part of the book concerns the meeting of the committee, on whose work the book is based, at the Pocantino Conference Center at the Rockefeller estate in Tarrytown, New York in September 1997. The meeting lasted four days and the people present were those who had investigated the UFO reports to be discussed and scientists who were there to ask questions and criticize the investigations.

The presentations of the Pocantino report include sections on photographs, luminosity data, aircraft and vehicle interferences, physiological effects, and analysis of debris.

In part three is the panel's response. There this reviewer came, for the first time in reading the book, to a feeling of disappointment. The panel's observations (page 122) are remarkably bland. The phenomenon should be studied and support given. Observers should be careful, for there is evidence that some observers have suffered radiation burns. The work supported by the French government is a model for studies in other countries. There seems to be a certain lack of forthrightness in the panel's recommendation, reflecting, no doubt, the fear that some on the panel may have had of being criticized by their colleagues, and even banned from the scientific community for having contributed to a study of a phenomenon still rejected by mainstream science.

In part four there are the post-Pocantino reflections. This is a series of chapters (20-24) by leading authorities in the field, which deals with some of the big issues (*e.g.*, is there a relationship of studies of the radio search for extraterrestrial intelligence (SETI) to those of UFOs). Part five contains five detailed presentations of "cases."

Incidentally, it is interesting to note that alleged "crashes" such as that of Roswell (1947), to which such close attention has been given, have occurred before and after 1947. In the book a number of them are described. This ends the book (with Chapter 29) and there follows then a brief guide to

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UFO literature, and a much needed list of acronyms.

This book, then, represents the first *major scientific* inquiry since the Condon Report. It should have a weight greater than that of the earlier account, because of the nature and reputation of the persons who composed it. It ought to reattract scientists to take part in an international effort which should be devoted to "meeting the challenge of the UFOs." The subject is obviously an ideal one for international collaboration on a large scale, particularly between authorities in the U.S., France, and Russia.

Unfortunately, life is constituted so that with most "good" there comes a corresponding "bad." Prof. Sturrock and his colleagues have bent over backwards—very far backwards—to present an utterly scientific, very high quality, presentation of the *Central Massif* of the great encircling mountain range which may stand as an analogy for the UFO phenomenon. However, in being so stringent, and in limiting the material discussed to physical evidence (where scientific methods can readily be applied), they have made the material so cut and dried that the significance of the findings to the inhabitants of the planet in general has been lost. Indeed, well-trained scientists who, in some fantasy, had never been exposed to the UFO phenomenon, but then are presented with this book, might well come away from this reading, putting it calmly down, and saying: "How interesting. . ." There is little sense in the writing to indicate that the most common opinion about the phenomenon is that the vehicles observed are extraterrestrial, for, if they are then we are on the point of meeting and greeting extraterrestrial beings. If that is to be the case, it will surely be a defining moment for the human race.

Thus, the UFO phenomenon is not simply a new phenomenon, here studied at a very scientific level, to be put away along with 1,000 other topics—finally, to turn up as a subsection in a physics book or perhaps a whole chapter in an astrophysics book.

There are a few examples of material which one would have liked to have seen treated in the book. There are reports of vehicles under water which are large and travel at high speed. A few of the abductee reports tell of people who have claimed to travel in UFOs under water, arriving finally at some under water "buildings."

Again, although the fly leaf does report the radar observations of a disk more than half a mile in diameter, the other data (some by airline pilots having observed objects the size of aircraft carriers) are not mentioned. Correspondingly, observations have been made of small vehicles which have been observed entering and exiting so-called "large mother ships."

Then, a huge area of putative significance lies in the work of John Mack, Professor of Psychiatry at Harvard University, on the accounts made under hypnosis of abductions; and corresponding accounts made by David Jacobs, Professor of History at Temple University in Philadelphia, of the corresponding work which he has done on ascertaining what people say about their alleged abductions, after they have been hypnotized. The material, which comes from these examinations, at first seems so mind boggling that one is again—as with the whole phenomenon—apt to stagger back and ask one's self whether it can be taken at face value. Readers of this review must make their own conclusions after they have read the very detailed and scientific accounts of Mack and Jacobs, respectively. Thus, Mack comes to the conclusion that the phenomena are fully real, his patients passed a number of tests for sanity, and the objectives of the extraterrestrials is to take genetic material from humans. Jacobs adds his reports to this theme, but prints part of his patients' descriptions, which support a much more sinister program. Thus, one of an alleged three groups of alien visitors has designs upon inhabiting the earth, a venture to be initiated with the aid of the "hybrids" whom they have been "breeding" for at least thirty years.

Another vein which might well have been discussed in the Sturrock report is the work of Phillip Corso, who was an Army officer at the time of the Roswell incident and wrote a book after fifty years (free, then, from official restraints) claiming that he saw and handled the bodies of aliens, and even giving reports of physical examinations of their bodies. He states that many post-World War II inventions came as copies of devices found on the Roswell vehicle.

Another aspect which one misses in the book, and which might have been included within the framework of physical evidence, is work from other countries. There are plenty of quotations in the book of the French work and some from the Swiss work, but surely most of the other countries have work on UFOs and much Russian work should now be available. It seems, in retrospect, unfortunate that at least one member of the committee was not funded to travel to these countries to see what he could dig up, particularly in the prevailing open atmosphere in Moscow.

Finally, the book avoids interpretation. Newton said, "I make no hypotheses" as to the nature of gravity. This attitude pervades the book, which doesn't even mention the several ideas which are current. The public decided decades ago (encouraged by movies such as the 1977 "Close Encounters of the Third Kind") that UFOs are small scout ships from the larger vehicles occasionally described. However, there are some very serious scientific objections to this hypothesis and it would surely have been most appropriate to come out with what they are; and if they make it unreasonable to accept the extraterrestrial theory, then what is theory No. 2, etc.

Prof. Sturrock and his colleagues clearly have been only too well aware of the danger of overstatement, but there may be here a greater danger of understatement. I feel an opportunity was lost at Pocantino in 1997, for it will not be easy to gather in one place such an able collection of people to state, at least, what may be possible.

In summary, this book will do most good if it can break through the crust of embedded skepticism in the universities, where talk of UFOs is on a par with a serious discussion of revealed religion. For the more general public, there is little that is new—accounts of the kind now blessed by the witnesship of Sturrock and his team have been described in the press and in many books for four decades. Goodness knows the subject is important enough and, it may be hoped, now has been dressed in a cloak of respectability.

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Zetetic Ruminations on Skepticism and Anomalies in Science

Marcello Truzzi*

Reprinted from *Zetetic Scholar*, August 1987.

On Zeteticism

When I first adopted the term *zetetic* to find an original name for a private newsletter dealing with scientific perspectives on anomalies, I thought that archaic term would likely avoid controversy. That was eleven years ago and mainly was the result of my looking for a term to replace the original title, *Explorations*, which some of my readers objected had already been adopted by other publications. Zetetic's dictionary meaning was simply "skeptical inquirer," and the last modern use of the term had been by the Flat Earth Society in the nineteenth century. But what started out to be an uncontroversial label soon emerged in my mind as perhaps the most controversial label of all. Through serendipity—some might argue synchronicity—I had stumbled on something now very significant to me, for I found upon researching the term that it both well describes and shapes my current orientation towards anomalies.

The term zetetic (both a noun and an adjective) was first applied to the followers of the Greek skeptical philosopher Pyrrho of Ellis (ca. 365-275 B.C.). Pyrrho urged suspension of judgement about facts, that we should "be without beliefs, disinclined to take a stand one way or another, and steadfast in this attitude."[1] But, as Richard H. Popkin has noted, Pyrrhonism should not be confused with Academic Skepticism that stemmed from Socrates' statement that "All I know is that I know nothing," the view that no knowledge can be certain. The zetetics took a more moderate position. As Popkin points out in his history of skepticism:

> The Pyrrhonists considered that both the Dogmatists and the Academics asserted too much, one group saying "Something can be known," and the other saying that "Nothing can be known." Instead, the Pyrrhonians proposed to suspend judgement on all questions on which there seemed to be conflicting evidence, including the question whether or not something can be known. . .The Pyrrhonian skeptics tried to avoid committing themselves on any and all questions, even as to whether their arguments were sound. Skepticism for them was an ability, or mental attitude, for opposing evidence both pro and con on any question about what was non-evident, so that one would suspend judgement on the question. . .Skepticism was a cure for the disease called Dogmatism or rashness. But unlike Academic skepticism, which came to a negative conclusion from its doubts, Pyrrhonian skepticism made no such assertion, merely saying that skepticism is a purge that eliminates everything including itself. The Pyrrhonist, then, lives undogmatically, following his natural inclinations, the appearances he is aware of, and the laws and customs of his society, without ever committing himself to any judgement about them.[2]

Like a proper zetetic, I remain uncertain about the ultimate correctness of this perspective, but as a working scientist, I find its practical function in avoiding dogmatism is most valuable. That is, this orientation is heuristic in that it emphasizes questions, rather than answers. It fits the aims of what Gunter[3] has called "Story Book" science (if not that actually practiced) while it avoids mistaking the goals of scientific method with science's current substantive content. But perhaps most important of all, I find this form of skepticism congruent with the fallabilism of modern philosophies of science and with the injunction of Charles Sanders Pierce that the foremost duty of the philosopher-scientist is to do nothing that might block inquiry.[4]

Regretfully, the term "skeptic" today is being used by many who adopt that label for themselves in a misleading way. To many, it is falsely equated with the term "rationalist." The dictionary meaning of the term indicates that a skeptic is one who raises doubts. Thus the word is meant to reflect non-belief rather than disbelief. But when we look at those who trumpet that they are skeptics towards claims of anomalies, we find disbelievers and debunkers rather than those who express uncertainty or doubt. The public "skeptics" of today present us with answers rather than questions. As philosopher W.V. Quine (himself, ironically, one among such modern public "skeptics") neatly made the distinction:

> It is important to distinguish between disbelief and non-belief—between believing a sentence is false and merely not believing it true. Disbelief is a case of belief; to believe a sentence false is to believe the negation of the sentence true. We disbelieve that there are ghosts; we believe that there are none. Non-belief is a state of suspended judgement; neither believing the sentence true nor believing it false.[5]

Of course, none of this is to suggest that disbelief is always in error or that there is not bunk that needs to be debunked. I only point out that disbelief should not be confused with skepticism and non-belief. This confusion is far from a new problem, and James H. Hyslop—who would probably disagree with Quine about ghosts—noted the confusion in a 1909 article in the *Journal of the American Society for Psychical Research* when he wrote:

> The average man today thinks he is a skeptic because he does not believe in a given allegation. The fact is that skepticism is not unbelief in the sense of denial nor in the sense of being opposed to a given belief, but it is critical igno-

But when we look at those who trumpet that they are skeptics towards claims of anomalies, we find disbelievers and debunkers rather than those who express uncertainty or doubt. The public "skeptics" of today present us with answers rather than questions.

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rance. Few men show this characteristic. They are too much ashamed of denying what they do not know something about. The public has gotten into the attitude of mind which it likes to call skepticism, but which is nothing more or less than dogmatism hiding under false colors. It thinks that belief is the only thing that can be biased and does not dream that denial can be biased, and in fact that the bias of denial is not only less justifiable but far worse than the bias of belief. It has no basis upon which to rest at all except belief. But people have come to think that denial or doubt is the mark of intelligence, when in fact true skepticism is much nearer being a mark of ignorance. True skepticism means that we do not know, not that such a thing is not true. To know that a thing is not true is knowledge, not doubt, and hence is subject to bias. It is all the worse when it parades itself as a trustworthy student of truth and in fact is only trying to deny it. The average mind assumes that belief disqualifies a man from studying a problem and that the only person who can investigate it is the man who does not believe anything about it. If the doubter has no opinions and is not biased by preconceptions of his own, and if he does not have an interest in an opposing theory, it is true that he may be better qualified than the believer to investigate, but the majority of those who parade as skeptics in the matter usually have some theory of their own to sustain against that which they claim not to believe, and hence are as much biased as the despised believer. . .[O]pen-mindedness is the only skepticism that can claim immunity from prejudice.[6]

Though Hyslop called our attention to this confusion seventy-five years ago, most anomaly researchers, instead of observing this distinction between non-belief and disbelief, seem to have accepted the critics' own definition of themselves as skeptics and constantly mis-refer to them as such. This has resulted in a most artificial polarization between believers and disbelievers, misrepresents both the options and the reality of the opinions held, and makes us zetetics—the true skeptics—either invisible or forced to choose sides or be thought of as "the enemy" by both sides.

In his now classic discussion of the normative structure of science, Robert K. Merton included organized skepticism along with universalism, communism, and disinterestedness among the institutional imperatives of science.[7] He referred to this as the "temporary suspension of judgement and the detached scrutiny of beliefs in terms of empirical and logical criteria," and then pointed out that this practice "may come into conflict with other attitudes toward the same data which have been crystallized and often ritualized by other institutions."[7] I would suggest to you that this conflict also occurs between one part of the scientific community and another. As our scientific institutions have developed, this becomes an internal as well as an external problem. And as these institutions have become integrated into other institutions, vested interests and nonscientific concerns (such as the control of economic resources) develop. And I suggest that as science grows into so called "Big Science," the norm of organized skepticism begins to conflict frequently with the norm of disinterestedness. This can lead to attempts by defenders of the majority or "orthodox" viewpoint to attempt to merely discredit rather than disprove competitive minority views (especially maverick claims), and this results in what Ray Hyman[8] has pointed out is a form of "pathological science."

True skepticism means that we do not know, not that such a thing is not true. To know that a thing is not true is knowledge, not doubt, and hence is subject to bias.

As Thomas S. Kuhn[9] has termed it, there is an "essential tension" within science since it must on the one hand preserve its accumulated knowledge by acting cautiously and conservatively, while on the other hand remain an open system ready to take in new and potentially revolutionary data and concepts. This balance is maintained through a number of methodological prescriptions which make it difficult but not impossible for the claimant of an anomaly to obtain acceptance of the claim. First, science places the burden of proof on the claimant. Second, the proof for a claim must in some sense be commensurate with the character of the claim. Thus, an extraordinary claim requires "extraordinary" (meaning stronger than usual) proof. This latter prescription seems related to the rule of parsimony in science that states that the simpler adequate explanation is the one to accept.

Now I would call your attention to the fact that these rather conservative rules for evidence of extraordinary claims mean that a claim that is inadequately supported results in a simple non-acceptance of the claim. Evidence is, then, a matter of degree, and not having enough results in a claimant's not satisfying the burden of proof. It does not mean disconfirmation of the claim. The proof is insubstantial and the claim is unaccepted rather than refuted. The claimant is, in effect, told either to give up or go back to find stronger evidence and arguments for a possible later day in the court of science. As a practical matter, an unproved fact is a non-fact. Science assumes the negative about unproved claims; it gives such claims low priority and low probability and ignores them. Since science is essentially descriptive (creating explanations through abstracted generalizations made about ascertained facts), it is not prescriptive.[10] Science can speak of the *highly improbable*; it can not properly speak of the impossible. But as a practical matter, the highly improbable is treated as though it were impossible. Working on a perpetual motion machine is almost certainly a waste of time, but once we deem it absolutely a waste of time, we close the door on such research and violate the equilibrium of the "essential tension" and disobey Peirce's injunction by blocking inquiry. The scientist who works on a perpetual motion machine may be playing the longest shot of all, and he may be conducting *stupid* science, but it is not necessarily false or pseudoscience.

Since the scientific community sets its priorities based upon the notions of probable pay-offs and importance of problems, the claimant of an unproven anomaly is playing a long shot and is unlikely to obtain scarce resources. And the unproven idea or anomaly may be, and usually is, ignored or treated as would be a false idea. So, I would suggest that this actually allows for greater tolerance than is often accorded claims of the extraordinary. If we really have faith in the scientific rules and the self-correcting mechanisms of science, we have little to fear from unusual low-probability or even crackpot ideas. We don't have to disprove an idea to discount it; we merely need to show its failure to bear its burden of proof. Further, I would suggest that concern with discrediting rather than disproving claims actually shows a lack of confidence in the normal adjudication system of science. Just as a proper system of justice

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does not require vigilantes, so does a proper scientific adjudication process not need any extra-scientific inquisition to protect us from alleged pseudoscience and so-called irrationality.

Organized skepticism in science is a two-edged sword. It allows us to question orthodoxy as well as unorthodoxy. Its proper functioning depends upon our tolerance and our ability to live with a certain degree of uncertainty and dissonance in our ranks. Robert K. Merton has spoken of the sociological ambivalence built into social roles.[7] The conflict by science's need for both conservation and openness—what Kuhn speaks of as creating the "essential tension" proper to science—demands the organized skepticism (so integral to science and which produces its self-correcting pattern of change that most of us think of as progress) which contributes greatly to that ambivalence. The tension has also been characterized as a struggle to avoid both kinds of what statisticians refer to as Type I and Type II errors. We don't wish to conclude something anomalous is out there when it is not, but we also do not want to overlook some rare anomaly, some weak signal amidst much noise.[11,12]

. . . there is an "essential tension" within science since it must on the one hand preserve its accumulated knowledge by acting cautiously and conservatively, while on the other hand remain an open system ready to take in new and potentially revolutionary data and concepts.

Most of my fellow critics of anomalies have characterized their own harsher and debunking-oriented approach as representing a "hard-line" skepticism, while my zeteticism is described as a "softer-line" variety. I think this misrepresents the reality of our differences. The public is indeed often misled, and we need to have critical views presented to them, to balance those of the proponents. Bunk needs to be debunked; fraud needs to be exposed; error needs to be corrected; and real pseudoscience—and it does exist—needs to be shown up for what it is. But fraud, error, and even pseudoscience have all existed within ordinary science, that is within legitimate, orthodox science. And much that gets labeled "pseudoscience" actually is not, for much so-called pseudoscience consists of scientific research programs (what I have called proto-sciences) being applied to investigate unorthodox or controversial matters.[13,14] I do not agree with those who equate science with a body of content; for me, science centrally remains a method. A science that studies unicorns may (and properly done, should most likely) find that no unicorns exist; but the search for unicorns can be conducted in scientific fashion. Critics forget that a person does not have to believe in ESP to be a parapsychologist or in UFOs to be a ufologist. Part of what science does is to determine whether or not variables exist; so to that degree, a science can investigate "nothing."

The reason that the zetetic may appear to take a softer critical line may be the result not so much of a less critical view of the extraordinary but of a more critical view of the ordinary with which it is contrasted. Those who leap to call parapsychology a pseudoscience might do well to look more closely at the social sciences in general. Those who laugh at the implausibility of a possible plesiosaur in Loch Ness should take a close look at the arguments and evidence put forward for the Big Bang or black holes. Those who think it unreasonable to investigate reports of unidentified flying objects might do well to look carefully at the arguments and evidence of those who promote current attempts at contacting extraterrestrial intelligence allegedly present in other solar systems. Those who complain about the unscientific status of psychic counselors should be willing to examine the scientific status of orthodox psychotherapy and make truly scientific comparisons. Those who sneer at phony prophets in our midst might also do well to look at the prognosticators in economics and sociology who hold official positions as "scientific forecasters." Those who concern themselves about newspaper horoscopes and their influence might do well to look at what the "real" so-called helping professions are doing. The scientist who claims to be a true skeptic, a zetetic, is willing to investigate empirically the claims of the American Medical Association as well as those of the faith healer; and, more important, he should be willing to compare the empirical results for both before defending one and condemning the other.

The skeptical posture of the scientist, I suggest, is one which T.H. Huxley termed agnosticism. Today, that term usually is thought to refer only to an orientation towards the question of the existence of a deity. But when Huxley first put the term forward, he meant it far more generally. And—note a moral for today—the term was meant to avoid the dogmas of both the Rationalists and the Religionists; in the Metaphysical Society of 1889, Huxley wrote:

> Agnosticism in fact, is not a creed, but a method, the essence of which lies in the rigorous application of a simple principle: That principle is of great antiquity. . . it is the fundamental axiom of modern science. Positively the principle may be expressed: In matters of the intellect, follow your reason as far as it will take you, without regard to any other consideration. And negatively, in matters of the intellect, do not pretend that conclusions are certain which are not demonstrated or demonstrable. That I take to be the agnostic faith, which if a man keep whole and undefiled, he shall not be ashamed to look the universe in the face, whatever the future may have in store for him.[15]

Huxley recognized that such tolerance was an ideal and not something he always was able to put into practice, for he went on to say:

> The results of the working out of the agnostic principle will vary according to individual knowledge and capacity, and according to the general condition of science. That which is unproven today may be proved by the help of new discoveries tomorrow. The only negative fixed points will be those negations which flow from the demonstrable limitation of our faculties. And the only obligation accepted is to have the mind always open to conviction. Agnostics who never fail in carrying out their principles are, I am afraid, as rare as other people of whom the same consistency can be truthfully predicated. But if you were to meet such a phoenix and tell him that you had discovered that two and two makes five, he would patiently ask

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you to state your reasons for that conviction, and express his readiness to agree with you if he found them satisfactory. The apostolic injunction to "suffer fools gladly" should be the rule of life of a true agnostic. I am deeply conscious of how far I myself fall short of this ideal, but it is my personal conception of what agnostics should be.[15]

Like Huxley, I think this a good ideal to uphold for the scientist, but it remains difficult to place it into practice. I therefore modify it somewhat. I contend that every scientist has a right to his/her own priorities. So, when a presumed crackpot approaches you with a wild idea, I do not think you are duty bound to listen and evaluate it. You have the full right to simply say you are too busy or to ignore such claimants. The claimant does not have a right to your time. But I contend that if a hearing *is* granted, it should be a fair and open-minded hearing based on consideration of the arguments and evidence. And if the arguments and evidence have some merits, even if they are inadequate to bear the burden of proof, we should be willing to admit such merits while still failing to accept (rather than necessarily denying) the claim. We must remember that evidence is always a matter of degree, and usually not overwhelming. And we must be willing to admit that even if we reject much evidence and argument, that there are likely to be "loose ends" that often can not be easily explained away. Most of all, we must remember that the goal of science is to explain rather than explain away phenomena. Skepticism should seek to be constructive, to advance science, rather than purely destructive and perhaps thereby to block inquiry.

Having now considered some of the characteristics of skepticism in science, let me turn now to the subject of anomalies.

On Anomalies

The dictionary definition of an anomaly as a "deviation from the common rule" or "something that deviates in excess of normal variation" is rather vague and somewhat misleading in that this definition also fits "irregularity" or "abnormality." As commonly used in the literature concerned with anomalies, the term emphasizes that an anomaly is something unexplained. An anomaly is something that is not covered by our current generalizations about how the world operates. It is something strange and unaccounted for. As we look at the term's use in the literature, it becomes rather multi-dimensional and is more like a constructed type or a fuzzy set.[16] In its pure state, an anomaly might be something that 1) actually occurs (that is, something both perceived and validated), 2) is not explained by some accepted scientific theory, 3) is perceived to be something which is in need of explanation, and 4) contradicts what we might expect from applying our accepted scientific models. In practice, an anomaly is often "impure" in terms of these four criteria. But I would suggest that 2), the anomaly's lack of fit with accepted theory, is the necessary element common to any real anomaly. It is a fact in search of an explanation.

This brings us to an important distinction. Coming to us from psychical research, the term paranormal (usually limited to psychological anomalies in psychical research) was created to designate phenomena considered natural—not supernatural—and which eventually should find scientific explanation but thus far have escaped such explanations.[17,18] The term supernormal has been used in somewhat the same way. The clear intention of those using the term paranormal was to avoid using the term supernatural. They wished to emphasize that the phenomena was aberrant but not beyond natural science; it was simply a matter of scientific explanation catching up with such maverick facts. Unfortunately, many critics of the paranormal continue to equate anything purportedly paranormal with the supernatural. This is particularly ironic since those who truly believe in the supernatural (such as the Roman Catholic church when it speaks of miracles) have long understood that a paranormal explanation precludes a supernatural one. Following the proper use of the term paranormal, we can logically speak of the parasciences which deal with anomalies in their respective domains. Thus, we can speak not only of parapsychology but also of such fields as parasociology, paraphysics, etc. In each case we refer to the study of anomalies within some discipline for which they seem potentially relevant.

In fact, however, given a particular anomalous event, we really don't know in advance which current science will eventually develop an explanation for it. Thus, something like extra-chance correct guessing scores may now be part of parapsychology, but in the end the explanation may lie elsewhere. For example, if it turns out that such anomalies are due to some statistical malassumption, that might cause us to develop some new statistical theory for the way such things are distributed in nature; the phenomenon may then better belong to parastatistics. Similarly, unidentified aerial phenomena may eventually be explained via meteorology, astronomy, psychophysiology, or some other discipline (or even by all of these in part since we may be dealing with different phenomena at different times). Thus, I would suggest the term put forward by Roger W. Westcott,[19,20] anomalistics, to speak of the interdisciplinary approach to anomalies of which the various parasciences are branches.

Returning now to the term paranormal, this term avoids our confusing true anomalies with the merely irregular or rare event that does have current scientific explanations. Such latter events are usually termed abnormal. Thus, a man eight feet tall would be abnormal but not paranormal, whereas a man who could breath water and live would be paranormal. Similarly, a virgin giving birth to a female would be abnormal (since parthenogenesis would offer an explanation for such a rarity), while a virgin giving birth to a male would be paranormal (to say the least!).

Since the key element in the definition of an anomaly is its relationship, or lack of relationship, to theory, this means that all anomalies are relative to some specific theory. The degree of anomalousness or extraordinariness of some event can only be specified in relation to the theory it fails to fit.

The scientist who claims to be a true skeptic, a zetetic, is willing to investigate empirically the claims of the American Medical Association as well as those of the faith healer; and, more important, he should be willing to compare the empirical results for both before defending one and condemning the other.

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The term anomaly specifies a relationship between an event and a theory; the event is not anomalous in and of itself. Science usually contains many theories, and they are by no means integrated or even consistent with one another. Thus, a claim like precognition may present serious problems for the physicist but few for the geologist. Not all scientists will view an event as equally extraordinary. This disunity may be functional, as when biologists ignored the physicists who said that the youthful age of the sun would make biological evolution impossible over the postulated great lengths of time. The physicists later discovered fusion and changed their estimate of the age of our sun, so in this case physics lagged behind the biologists' theories even though we usually think of physics as the more fundamental science.

Anomalies are actually the major source for theoretical change and conceptual progress in science.

I find that in the broadest sense there are three general orientations towards anomalies. For many scientists, anomalies are mainly troublesome and to be ignored or denied. They view anomalies as what Charles Fort called "damned facts." For such scientists, anomalies are, at best, an irritant. At worst, they view anomalies as challenges to what is fundamentally held dear and so are to be opposed, sometimes at almost any cost. The object, for such scientists, is not to explain but to explain away the anomaly.

The second orientation commonly found is that of the mystery monger, the person who relishes the fact that a phenomenon is unexplained. This, alas, is quite frequently the attitude held by many who call themselves "Forteans."[21] They enjoy the mystery and even gloat over the inability of scientists to account for the anomaly. They don't want to explain or explain away anomalies; they want to uncover more of them, and their goal may—in the extreme case—be anti-scientific, in spirit, for they seek mysteries rather than explanations of them. In reading some Forteans, one clearly gets the impression that even an unconventional normal explanation by science would be a disappointment.

The third orientation—the one I espouse—consists of seeing anomalies not merely as challenges but as opportunities. Anomalies are actually the major source for theoretical change and conceptual progress in science. Though we should be highly cautious about accepting claims of anomalies, we should look forward to finding valid ones because it is by explaining these aberrations that we will expand our theories and create new theories. From this perspective, anomalies are important and valuable because they lead to new and greater forms of scientific explanation. Thus, anomalies should be seen as constructive rather than destructive, as forces inspiring scientific growth and progress.

Let me turn now to some of the various dimensions or categories of anomalies.

1) The anomaly may be legitimately unexplained or paranormal rather than something at first inexplicable but actually covered by known laws, something merely abnormal (as just discussed) or a pseudo-anomaly.

2) The anomaly may be a scientific anomaly, something paranormal and ultimately explicable in terms of science, as opposed to something actually outside the natural order, something not paranormal but supernatural or metaphysical. The latter would include not only such things as divine miracles but also perhaps any possibly unique event in nature, a kind of cosmic hiccup or burp, something without pattern or impossible to generalize about or find lawful order in. Science is law seeking, and it is at least conceptually possible that some things may happen that are simply outside all regularities, some things I would term preternatural.

3) The anomaly can vary in its relationship to scientific theory. Thus, it can be unnested, that is, simply a stray phenomenon that seems to have no body of theory to deal with it one way or another. Or, more typically, the anomaly can be nested, that is, can pretty clearly be seen as something that should be covered by a certain theory even if it is not. Nested anomalies can vary greatly in the degree to which they fail to fit within the existing theoretical net. Some anomalies merely fit poorly while others seem to contradict central parts of the theory. The nested anomaly that contradicts important parts of the accepted body of theory relevant to it is the one most troublesome and most likely to be denied legitimacy/validity.

4) An anomaly may be something spontaneous in nature or may be something which only can be experimentally produced. And if it is experimentally producible, it may vary in the degree to which it is reproducible. Replication can be occasional only or vary all the way up to repeatability upon demand. In general, anomalies tend towards being spontaneous or only occasionally repeatable, and that is usually one of the reasons their very existence is typically controversial.

5) Anomalies exist to some degree in every scientific area and are usually acknowledged as such. Thus, there are accepted anomalies which are not controversial. These are usually viewed as minor puzzles to be solved, as not very troublesome loose ends. But most anomalies deemed interesting have a struggle to gain acceptance. Most anomalies are merely alleged anomalies. They remain unestablished. Some are validated anomalies in that most scientists might agree that they really exist; but this is unusual, and it is imperative that the anomalist recognizes that most of the time he is dealing with merely alleged anomalies.

6) An anomaly may be something that occurs rarely in nature or something that occurs frequently, even if it is a spontaneous anomaly. Frequency must be considered relative to the temporal dimension. Thus, something may appear to be rare but may actually have occurred on many occasions over a long period of time or, conversely, an event may occur a number of times within a short period but never happened previously or afterwards.

7) An anomaly may vary along the spatial dimension. That is, an anomaly may be widely dispersed or narrowly available.

8) An anomaly may be rarely or frequently seen (whether or not it occurs rarely or frequently).

9) An anomaly be rarely or frequently reported (whether or not it is rarely or frequently seen).

10) The anomaly may be an anomalous thing, or it may be an anomalous relationship between quite normal things. I have elsewhere termed anomalous things crypto-events and anomalous relationships para-events.[14] Things, crypto-events, are relatively easily validated. You simply "bring in the body" and show it to the skeptics. But falsification of the crypto-event may be especially difficult since the thing (*e.g.*, a Bigfoot) could have been elsewhere than where you looked. The reverse is

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true for the para-event. Since a relationship is something to be inferred from the data, alternative explanations are usually possible. Thus, validation for any para-event is most difficult. But we can usually agree that a certain correlation from which we would make our inference of the para-event should be obtained, in a given experiment. So, if we fail to find it, that is usually accepted as a fallification of the claimed para-event. Anomalistics, then, consists of both cryptosciences (which study hidden things, *e.g.*, as cryptozoology studies anomalous animals) and parasciences (which study anomalous relationships between things, *e.g.*, as parapsychology studies anomalous psi processes), and these two branches of anomalistics have different strategic problems as they seek acceptance/ legitimacy within the general scientific community.

11) An anomaly may appear bizarre or mundane. From a scientific standpoint, this *should* largely be irrelevant. But it clearly makes a difference to the way many will evaluate the degree of extraordinariness of the claim. An anomaly high on strangeness will have more trouble getting accepted even though it may have less trouble getting noticed in the first place.

Having considered some of the major dimensions of anomalies, let me now try to integrate some of these observations with my earlier remarks about skepticism.

The Zetetic Approach to Anomalies

When speaking of the extraordinary in science, we need to consider both extraordinary events and extraordinary theories (explanations). We should first seek to explain extraordinary events in terms of ordinary theories, and only upon failing should we move on to extraordinary explanations.[17] But, alas, the anomaly literature is full of far too many attempts to explain ordinary events with extraordinary theories.

Any anomaly claim will normally consist of three elements: 1) the anomalous event itself, 2) the report or narrative about that event, and 3) the reporter or narrator of the report or narrative account.[17] Critics can attack the claim at any or all of these three points. The event can be viewed as too improbable, the narrative as too implausible, and/or the narrator as not credible. Each of these elements can be assessed independently, and the skeptic usually begins with doubts about the claimed event itself. He then properly focuses his arguments and evidence against the narrative account. But it is common for criticism to extend to the credibility of the narrator, this sometimes taking the form of irrelevant *ad hominem* attacks on the narrator's motives.

Attacks on the credibility of the narrator follow a pattern. The reporter of the anomaly can be attacked through the use of a number of negative labels. He can be called a crank, a crackpot, an incompetent, or a charlatan. All these labels have been used to designate what the critic sees as a "pseudoscientist" (far too often a magic word invoked to exorcise heresy). A crank is simply one who tenaciously clings to a deviant or minority position. This is normally done via rational arguments and evidence. We have many cranks in ordinary as well as extraordinary science. Ron Westrum has pointed out that there are both *reactionary* cranks (those who cling to old, discarded ideas) and *radical* cranks (who espouse wild new ideas). Cranks are "difficult" persons (cranky?) but not irrational. A crackpot is one who supports a wild idea (also either reactionary or radical) but does so irrationally, *i.e.*, without proper arguments and/or evidence. He is a scientific "nut." The terms *crank* and *crackpot* represent references to the argumentative style of the proponent so labeled.

An incompetent is simply one who makes unintentional errors, out of inability or ignorance. Ordinary science is full of incompetence, as is any professional field. But a charlatan is one who makes intentional errors; he is a fraud.

When the proponent of an anomaly is an insider in science (what Isaac Asimov has termed an endo-heretic), critics tend to be more tolerant and usually pull punches and reduce even the crackpot or charlatan in our midst to mere crank or incompetent. But if the proponent is an outsider (an exo-heretic), critics tend towards exaggeration, elevating the mere crank or incompetent into the role of a crackpot or charlatan.

We might also note that critics may themselves be members of the science community (what I would term endo-critics) or outsiders such as philosophers, science writers, or even magicians (exo-critics). Exo-critics seem to be even more concerned with social control over the boundary between science and pseudoscience than endo-critics. And such exo-critics may play the role of what Ray Hyman has termed "hit men" in science, brought in to discredit deviant claims and claimants and to "protect the public." At its worst, this takes a holier-than-thou (more-scientific-than-thou?) posture, and such vigilantes can act rather like inquisitors for a Church of Scientism.[22]

It has been observed that acceptance of an anomaly can come about through either 1) an adequate degree of replicability (for some critics of some claims this may mean repeatability upon demand); 2) a new acceptable theory to house the alleged maverick fact, thereby reducing or eliminating its anomalous character; or 3) some practical application of the anomaly (a purely pragmatic and atheoretic approach). Number3) is actually a version of Number 1) since usefulness implies repeatability, but the level of adequacy in replication would be lower for (1) since the atheoretical argument side-steps contradiction of existing theories.

If extraordinary claims require commensurate "extraordinary" proof, the requirements (in terms of quality or quantity, not in terms of the standards) diminish with a reduction in the extraordinariness of the claimed anomaly. And if we are to judge *how anomalous* a claim is, we first need to explicate clearly what impact the existence of the anomaly would have on our theories. An unnested anomaly should require less evidence than a nested anomaly. If we look closely, we frequently find that a common view of how anomalous something is may be confused with how "strange" or "bizarre" the claim may seem. Possibility of the event is confused with plausibility of the narrative.[17] Almost no one believes in unicorns today, but finding living unicorns would have very little impact on current zoology. Telepathy would be far less revolutionary for psychological theory than would be precognition, so telepathy *should require* less proof. And a living plesiosaur in Loch Ness would be far less extraordinary a claim than would be a living mermaid or a centaur. We need to far more carefully explicate how scientifically extraordinary a claim really is and then weigh the evidence in light of that assessment.

It should also follow that claimants can reduce need for evidence by minimizing the revolutionary character of their claims. This can usually be done by taking a more atheoretic approach when presenting evidence. Like Sherlock Holmes, we should eliminate the unnecessary elements and consider the bare bones first. We especially should try to dissociate our anomaly from any occult or metaphysical frameworks (even if these inspired our investigation). Parapsychologists keep reminding us that they are not occultists and are not part of Spiritualism. Cosmobiologists remind us that they are not

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arguing for traditional astrology. Some ufologists remind us that they are investigating *unidentified* aerial phenomena and are not proponents of *identifying* UFOs as extraterrestrial craft. In general, this means any anomaly should first be presented as a question rather than as part of some extraordinary answer. Exotic correlations, for example as in Michel Gauquelin's "Mars Effect" or in parapsychology's extra-chance guessing scores, should first be presented as mere correlations before leaping to conclusions about causal relations.[23] Too often what might have been accepted as a legitimate puzzle is repudiated because the claimant insists he has found a fulcrum to place his lever for scientific revolution and paradigm shift.

But the claimant finds himself in a paradoxical position. By thus reducing his anomaly claim, he may trivialize its importance, and that results in its being assigned a lower priority for scientific investigation. So, the claimant is usually forced to seek attention and resources by stressing the revolutionary character of his claim. But the more important the anomaly is in this way, the greater is the degree of proof demanded; and raising the threshold for adequate evidence makes criticism easier. This often forces the proponent of the anomaly to seek support outside of science, either from the public or—worse—from occult or metaphysical supporters; this then further mobilizes antagonism among scientists.

But if the proponent is trapped in this spiral, surely the critic is not. The goal of the scientist is to explain whatever anomalies truly exist. So the critic—independent of the proponent's posture—has an obligation to examine the strongest evidence for the least theoretic version of any anomaly claim.

Finally, we need to recognize that the importance of an anomaly may be primarily extra-scientific.[24] Psychologists may properly assign low priority to parapsychological research given the level of proof so far offered. But the military and security-concerned branches of government may recognize that even such long shots would have enormous impact if even only partially valid. So, scientists may rationally assign low or high priorities based on their specific reasons for wishing to avoid a Type I or a Type II error, and opposite assessments of priority can rationally emerge from different orientations and stemming from nonscientific factors. Critics of government research into anomalies sometimes neglect to consider such factors. But so, alas, do some proponents. Thus, covert government interest in UFOs does not necessarily mean greater covert belief in the reality of UFOs. It may merely reflect government's different criteria for the importance of avoiding a Type II error. It may even be the case that such misunderstanding is partly the reason that government agencies feel obligated to keep their work covert. A similar argument applies to covert government work in parapsychology; such work does not necessarily indicate covert government belief in the reality of psi.

I have tried to consider some of the interrelationships that emerge from applying a zetetic approach to anomalies research. As always with proper zeteticism, I leave you with more questions than answers about these matters. But I think these are some of the right questions that need to be asked if we are to make progress in dealing with scientific anomalies.

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What Really Happens in Bell Correlation Experiments?

Caroline H. Thompson*

Revised lecture (after peer review) from the June 2000 Natural Philosophy Alliance Conference, held in Storrs, Connecticut.

Abstract

Do any real experiments demonstrate "non-local" correlations? Is apparent "quantum entanglement" evidence of faster-than-light effects? As I shall attempt to show, there are other possible explanations for the observations, and these involve nothing more sensational than classical ideas about the nature of light, shared information from the source, and realistic hypotheses as to how the apparatus works. The experts themselves admit that there is so far no valid evidence that the kind of entanglement that seems to require non-local actions ever actually happens. Yes, "Bell tests" are violated, but they are not *valid* versions of Bell's test, so they do not rule out local realism.

I shall attempt to explain what the debate is all about, and how the real experiments can be modeled without any need to invoke quantum weirdness. The weakness of the quantum theory case is known to those in the field—one sees occasional references in the literature to "loopholes." These, as it turns out, are the key to the realist solutions. Faith in the universal success of quantum theory leads editors, referees, and the experimenters to believe that they will eventually be closed, and that meantime science is best served by choosing the quantum theory interpretation of the results. But in my view they are perpetuating a myth, one that is undermining science by the introduction of magic.

Introduction

I am not alone in my discovery that the actual "Bell test" experiments, conducted from about 1969 to the present, do not support quantum theory nearly as convincingly as is commonly stated. I propose to try and explain a few of the ways of arriving at this conclusion. The waters are murky, though. We have to deal with several variants of the Bell test, various different kinds of experiments, different ways of treating the data, and various misconceptions held by prominent people. There is a vast quantity of confusing literature, mostly with little bearing on my topic: the actual experiments. I shall try to tell you what I think really happens, but this is not something to be understood in five minutes! I hope, nevertheless, that the later sections of the paper will convince even those who remain bemused that the experimental evidence should not be taken on trust.

In 1993, spurred on by a book review in *New Scientist* and with no formal training in physics, only in maths and statistics, I began to study Alain Aspect's experiments[1] on the EPR (Einstein-Podolsky-Rosen) correlations—experiments using Bell tests. As it turned out, my training in statistics was invaluable. I had experience in design and analysis, having been employed for several years as an independent advisor to experimenters. Within a few weeks I had discovered for myself some of the suspect assumptions and consequent possible realist explanations, later finding out that almost all had been explored already by people such as Marshall, Santos and Selleri,[2] Gilbert and Sulcs,[3] and Wesley.[4]

In the course of the next few years I had considerable correspondence with many people in the field. Though I failed to get my articles into the mainstream journals, I have not been entirely ignored. I have received encouragement to some extent from those actively involved, as well as from other "dissidents" (some of this is reported in an article[5] in *Accountability in Research*). It emerged that the "loopholes" were well-known to all concerned, including the editors and referees of *Physical Review Letters* (PRL) and *Physical Review A* (PRA). They have faith, though, that the same results *would* be obtained if all were closed. I have equal faith that they are wrong, and that the results that would prove this have so far been ignored. The investigations that could have clinched the argument have either never been done or have been left unreported.

There are plans to conduct "loophole-free" experiments using atoms[6] instead of the "photons" that have mostly been used to date, but these are invariably very complicated and highly dependent on theory. We might learn more by re-examining the existing schemes, seeing if we cannot improve our whole experimental approach. The exercise might, incidentally, help us to understand the nature of light, rejecting (for low-energies at least) the concept of the "photon" and returning to a more classical pure wave model. Those who insist on the "photon" will find some of my ideas unacceptable. By ruling out most realist explanations, however, they will have little choice but to fall back on the action-at-a-distance interpretation of the experiments—this despite the total absence of any proposal for its mechanism. This mechanism, by the way, has to act not only in the laboratory but along several kilometers of fibre-optic cable.[7]

What Are Bell Tests All About?

For a full treatment of the subject, the reader is referred to books such as the recent one by Afriat and Selleri,[8] which includes, incidentally, a few pages on my own work. The original EPR argument[9] was intended as a challenge to mathematics and logic of quantum mechanics (QM). It concerned the possibility that a pair of particles, A and B, that had once interacted might remain correlated in a manner that conflicted with Heisenberg's Uncertainty Principle. By measuring momentum on one and position on the other, you might deduce both to high accuracy, which, of course, Heisenberg's rule disallows. This may be of academic interest, but something much more important is also at stake. The authors were investigating the consequences of the QM assumption that the system could be described by just one "wave function," which was not "separable" (see following explanation). The system evolved—and the particles encountered various pieces of experimental apparatus—as a

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single system, not able to be considered as two independent parts. The unfortunate consequence of this, the mathematics said, was that the *setting* of the apparatus measuring A could affect the result of the measurement on B. The effect would be instantaneous, "non-local."

["*Separable*," in the context of the actual experiments, means effectively the same as "*factorable*." It is the consequence of the "local realist" assumption that the detections of the two separated particles are independent events and any correlation between them is fully accounted for by "hidden variables," λ, conveying shared information from the source. Once λ is fixed you can obtain the probability of detecting both "particles" (in practice, both light signals) by multiplying the individual probabilities. In other words, the joint probability, $p_{AB}(\lambda, a, b)$ "factors" into $p_A(\lambda, a)\ p_B(\lambda, b)$, where a and b represent the settings of the two detectors.

Thus the basic realist model says that the expected average probability of a joint detection can be expressed as:

$$P(a,b) = \int d\lambda\ \rho(\lambda)\ p_A(a,\lambda)\ p_B(b,\lambda) \qquad (1)$$

Integration is performed over the complete "hidden variable" space spanned by λ, which may be of any number of dimensions. The weighting factor, ρ, represents the relative frequencies of the different "states," λ, of the source. Note well that it is the *integrand* that factors, not the integral.

In practice, in the early EPR experiments at least, there is nothing mysterious about λ. It is simply the polarization direction, with a possible second component, the amplitude.]

At the time, there was heated debate, opposition to EPR's challenge being led by Niels Bohr.[10] The realist assumption is the common sense one, that the system *is* separable. The version of QM that Bohr was supporting (and he was very influential!) said that it was not. But there is no obvious reason for this. Few have pretended to understand Bohr's reasoning. Why should you not be able to assume the standard result, that independent probabilities are multiplied in order to get joint ones?

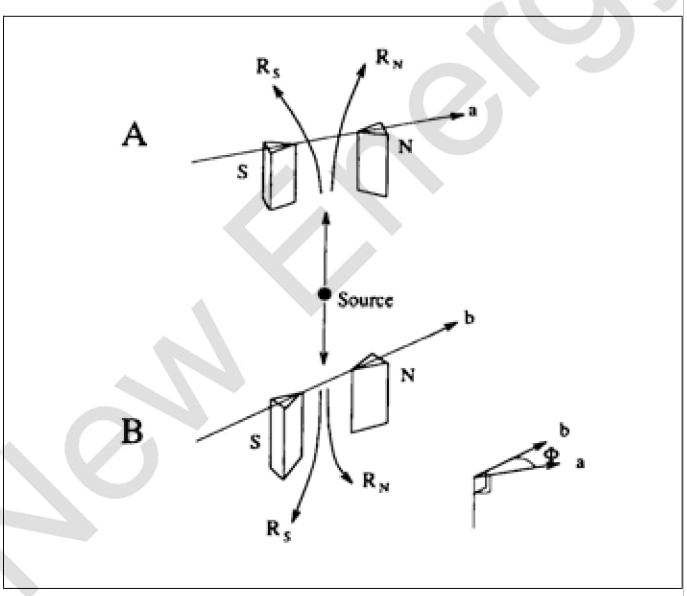

Figure 1. Scheme for a Stern-Gerlach EPR experiment. N.B. This has never been done! Note that the diagram illustrates one of the important features of "quantum" measurements: we do not measure the direction of spin of each atom, only, indirectly, the component of it when the detector magnets are set at some chosen angle.

As Furry[11] pointed out in a paper published in 1936, "the assumption that a system when free from mechanical interference necessarily has independently real properties is contradicted by QM." In Expression (1), λ represents these real properties, or those of them that are relevant and were determined at the source. QM is incompatible with their existence. The theory implies that, however far a pair are separated, they will influence each other, inducing correlations that cannot be accounted for in this manner.[12]

This is what the EPR problem is all about. Opinions differ as to whether we are talking about real influences or only apparent ones, but the fact remains that most people in the field go along with the idea that the experiments back QM. QM's reputation for giving uniformly correct predictions has no exception. [If you find the argument already confusing, you are in good company! A great number of people with whom I have discussed it find that they just cannot believe that the two statements, that "the particles are correlated" and "the detection events are independent, so that you can multiply probabilities once you've fixed λ," can both be true. In the abstract, there seems to be a problem, especially if you are introduced to the subject in terms of "deterministic" models, and try to make λ *completely* determine the outcome. This can be done but it makes life more difficult. The problem becomes considerably more manageable if λ is defined so as to determine only the *probability*. Look at the structure of the integral in Expression (1) or at the real experiments and this begins to make sense.]

The majority of investigations into the EPR challenge deviate considerably from the original idea. They do not mention position or momentum but, instead, pairs of more closely-related variables such as "spins" of atoms measured from two different angles. A great many have been thought-experiments only, evolving from a proposal of David Bohm's[13] of around 1950. He suggested that something similar to the EPR hypothesis could be tested using two "Stern-Gerlach" magnet setups to measure the spins of two atoms that had once been part of the same molecule (see Figure 1).[14]

Now, at last, John Bell enters the story, and with him the possibility of real experimental tests. In the mid 1960s he devised a statistical test, soon to acquire his name, for use in Bohm-type experiments. It would discriminate between separable (factorable) and non-separable models, and hence between quantum theory and local realism. A set of experiments was needed, measuring coincidences—simultaneous detections on A and B sides—for a few selected pairs of settings. If Bell's test statistic is exceeded, this indicates that "nonlocality" is a fact of this real world. [When you read that the actual experiments can only be explained if there are instantaneous influences between the two detectors, this is by no means a direct conclusion! All that is actually seen is that the test statistic seems regularly to come out larger than it should, if formulae of type (1) above hold. But it is only a test statistic! It depends on the data, which depend on the apparatus and on any transformations and adjustments that may have been made. In reality, there are a great number of free parameters available, and hence a number of other possible rea-

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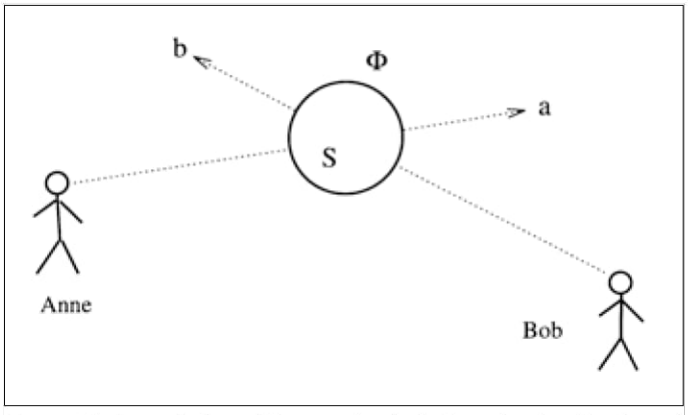

Figure 2. Anne, Bob and the moving ball. S marks the South end of the magnet (N is out of sight); a and b are directions in which the assistants are viewing the ball; Φ the angle between them.

sons for that high "Bell test" statistic, appearing to support QM.]

To continue our story: Bell tests evolved towards the end of the 1960s into "CHSH" (Clauser, Horne, Shimony, and Holt) tests,[15] and, more recently, into a deceptively simple one: that of "visibility." (Reader beware: there is little agreement on nomenclature for the various tests!) The modified tests were needed because nobody has been able to devise a satisfactory way of conducting Bohm's experiment using atoms, so they have performed instead what they believe to be equivalent ones using light. There is an important difference, though. Whereas with atoms it might reasonably be supposed that you detect every single one, with light it is well-known that we do not have "perfect" detectors, able to register every "photon." (In a purely wave model of light, this is inevitable, and there is no prospect of ever achieving this kind of perfection.) The variants of Bell's test devised by Clauser *et al.* all required auxiliary assumptions. Neither the officially recognized ones nor the many others implicit in the experimental schemes (many of which are hinted at in footnotes to Clauser and Horne's 1974 paper[15]) have received the attention they deserve.

Some of the tests—the majority of those used in practice—are not valid unless one knows the number of photon pairs emitted. It is possible to make a strong assumption—that of "fair sampling"—that makes them usable, but this is not, unfortunately, as reasonable as it sounds. The kind of fair sampling that is involved is not something under the experimenters' control. It is associated with the inefficiency of those detectors, and with the "detection loophole," which has been known since 1970, if not before.[16]

In this paper I shall concentrate on the two factors that are now widely accepted as potentially invalidating the tests: the inefficiency of the detectors and the subtraction of "accidentals." I shall not strain your patience by telling you about others, such as the failure of "rotational invariance" or possible problems with timing.[17,18]

The Chaotic Ball Model

The principle behind the detection loophole is illustrated in a paper I published in 1996.[19] I shall not go into details here, but just present enough to give you the flavor of my argument. It is based around the analogy of two assistants looking at a moving ball (Figure 2), in which is embedded a magnet. "Detections" correspond to the cases in which the assistants can see one or other end of the magnet. They will not see quite the *whole* surface (Figure 3)!

Now the "standard" form of Bell's test (the one of form $-2 \le S \le 2$) depends on the difference in frequency between "SS or NN" and "SN or NS" records. But how are we to estimate these frequencies? In this simple analogy it is clear that, in the limit, they will be proportional to the shaded areas, but what do we divide by to convert observed numbers to frequencies? If we knew (as indeed in the analogy we do!) the number of pairs we started with, clearly this is what we would divide by. It would be equivalent to the total surface area of the ball. But in practice this number is not known. Experimenters divide instead by the total number of observed coincidences, or in other words by something equivalent to the total shaded area. And they manage to infringe the "Bell test"!

The model makes it very clear what has happened. They are dividing by a quantity that is too small. In fact, it is not hard to see that the statistic they are actually using has an upper limit of 4, not 2. Despite the fact that a slightly different geometry is needed to model optical experiments, this is equally true here, and when dealing with "detection efficiencies" known to be of the order of only 5%, there is clear potential for trouble.

Ignoring the detection loophole, or making the fair sampling assumption, amounts to affirming the belief that if there are non-detections they are spread evenly over the surface of the ball. Although in optical work this is perhaps not quite as unreasonable as in my analogy, I can see no scientific justification for it. The loophole invalidates the test, yet it or its equivalent continue to be used, the results quoted as disproving local realism!

My chaotic ball is based on Stern-Gerlach-type experiments, which have never been done. It is possible to adapt it to cover the actual experiments using light, but perhaps easier to see the logical fault in the tests used for these if we start again.

An Actual Bell Test

Let us consider one of Alain Aspect's experiments—his first, reported in 1981 (see Figure 4). This experiment differs from the setup discussed so far in that (a) it uses light, so that it is not so reasonable to assume a deterministic model, and (b) the analyzers (polarizers, P_A and P_B of Figure 4) each have

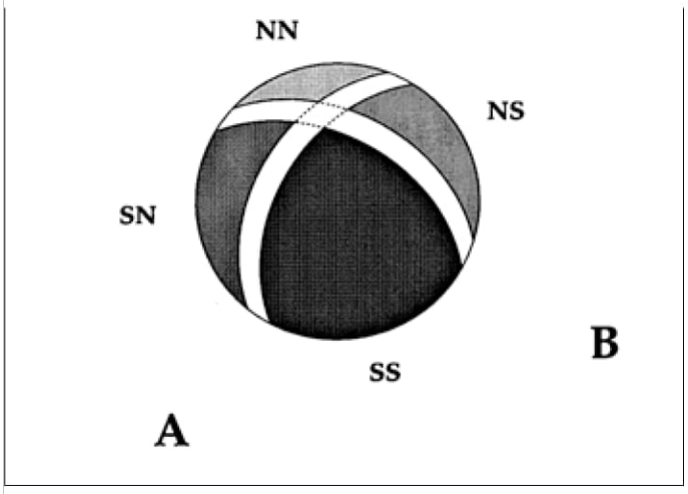

Figure 3. The registered coincidences: Chaotic Ball with missing bands. The first letter of each pair denotes what Anne records, the second Bob, when the S is in the region indicated. There is no coincidence unless both assistants make a record, so some data is thrown away.

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only one output channel. With only the one channel, the standard Bell test is not applicable, but I shall leave discussion of the actual test used to my papers,[20] discussing here only the "visibility" test, currently in vogue.[21]

In this experiment, the hidden variable is "polarization." Each pair of emitted light signals has a common polarization direction, at angle λ. When the signals pass through the polarizers, "Malus' Law" tells us, using classical theory, that the intensity is reduced by the factor $\cos^2(a - \lambda)$ or $\cos^2(b - \lambda)$ as appropriate. *If* (but see later!) our photodetectors (D_A, D_B) are obeying the laws of quantum mechanics, it follows that the probabilities of detection will follow these same laws.

If (and again, this is not to be taken for granted!) all λ values are equally likely, we can immediately translate Expression (1) into an actual prediction for the coincidence rate:

$$P(a,b) = 1/\pi \int_0^{\pi} d\lambda \cos^2(a-\lambda)\cos^2(b-\lambda)$$
$$= 1/8(1 + 2\cos^2(a-b)) \qquad (2)$$

Aspect himself used a "CHSH" Bell test on his data (see my paper, reference 20), but, for simplicity, I shall talk here about the modern test, which uses the visibility ((max – min)/(max + min)) of the coincidence curve.[22] This is a test that is almost equivalent to the standard Bell one, and it declares that visibilities in excess of 0.5 cannot be accounted for by realist models.

Consider, however, the properties of the model represented by Expression (1). It translates into Expression (2) *if* we assume detectors obeying quantum theory, but what if our detectors in the real situation do not? There is no logical reason for the *probabilities* to obey $\cos^2$ curves, and in fact there is good reason to suppose that they never obey them.[23] To have high visibility, the curve has to have a low minimum, and if we keep to $\cos^2$ laws, and assume uniform distribution of polarization angles, then the minimum is in fact 1/8, which leads to the 0.5 figure in the test. If, however, we replace the law by one that has a broader trough than $\cos^2$, we can get a lower minimum and higher visibility! The test is not at all robust. In my view it is totally unreasonable to consider only realist models that involve perfect detectors and then, when visibilities are observed to be high, declare that you have ruled out *all* of them! But, in point of fact, this 1981 experiment did not infringe either the Bell test actually used or the visibility test until after adjustment of the data.

Data Adjustments

All Bell tests, including the visibility test, are biased towards quantum theory if you adjust the data by subtracting an approximately equal quantity from every observed coincidence rate.[24] Such an adjustment has frequently been performed, in the guise of subtraction of the "accidental coincidence rate." This is the feature I have concentrated on in several of my papers, including those of reference 20. Partly, this is because it is of historical importance, since it was done by Alain Aspect (in his Ph.D. thesis[25] he goes to some lengths to try to justify it) but also because you do not need to know much about the experiments or the Bell tests to be able to see that there is room for doubt here.

The main data that I discuss in my papers is that from an unsorted table (VII-A-1) in Aspect's thesis. My summary is given in Table 1. A graphical representation (Figure 5) reveals clearly that with no adjustment the data is closer to the realist prediction than to the quantum theory one!

Though my attempts at getting a paper published on this subject in PRL or PRA have failed, my campaign to get the practice stopped has had some success. I do not think you will find future experimenters who use Bell tests or the equivalent will be subtracting accidentals without at least making it very clear that they have done so.

How to Prove Realism is Right!

There are tests that could be done and have not been done, or, at least, not been reported. Aspect himself realized that if he had reduced his emission rate he ought to have been able to reduce his accidental rate to negligible values without reducing his "true coincidence rate" so much. With low emission rates he should have been able to show the same infringements of Bell inequalities without needing to do any subtraction. This should have been done. Aspect found some "anomalous" results (a few are mentioned in my Chaotic Ball paper), and reported them in his Ph.D. thesis but not elsewhere. I think they all deserve further investigation.

Regarding the detection loophole: the response charac-

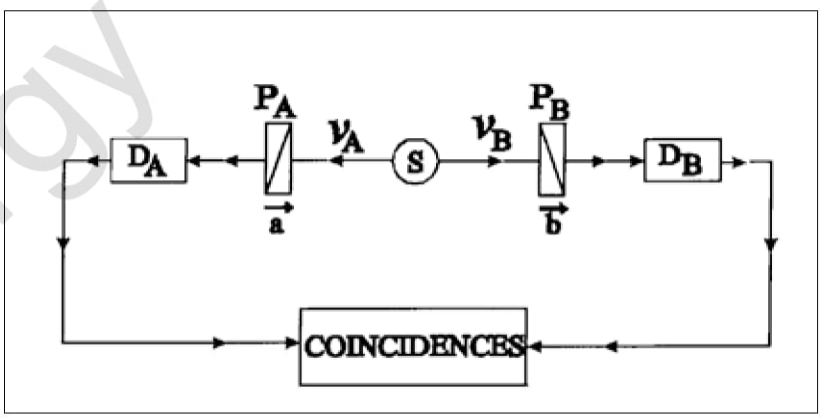

Figure 4. Aspect's first EPR experiment, 1981. Light from the source, S, is filtered so that wavelength ν_A can proceed to the left, ν_B to the right.

Table 1: Raw and adjusted data for Aspect's 1981 experiment.

Angle between polarisers	0.0°	22.5°	45.0°	67.5°	90.0°	One polariser absent	Both absent
Raw coincidence rate	96.0	86.8	63.8	38.3	28.0	126.0	248.2
"Accidental" rate	22.6	22.8	23.0	22.5	23.3	45.5	90.0
Adjusted rate	73.4	64.0	39.8	15.8	4.8	80.5	158.2

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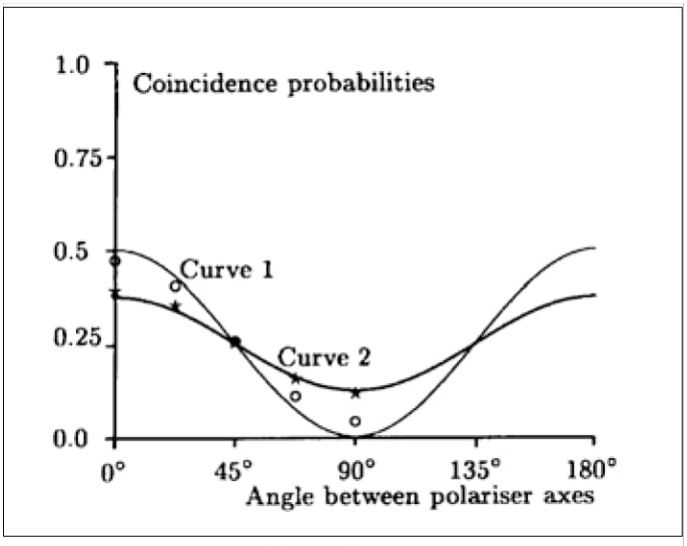

Figure 5. Realist and QM predicted coincidence curves, and Aspect's 1981 data. Curve 1 is the QM, curve 2 the realist prediction, both for the ideal case. * Indicates raw data, ° adjusted.

teristics of detectors should be investigated, and EPR experiments repeated using a range of emission intensities and detector settings so as to see how the curves and the test statistic vary. I would expect to find that there are cases in which the supposedly sinusoidal curves produced are not truly sinusoidal. (There are hints of this in some published graphs.)

There are many parameters that can be varied, and the law of "causal inference"[26] would, I believe, show that the quantum theory model followed the wrong logic, the realist model the right one.

So What Has Gone Wrong?

The scientific community seems to have gone off on the wrong track. Why? Why have the experimental checks not worked? I could (and may in the near future) write a whole book on the subject. I'll just suggest a few reasons here.

1) The human mind is not well-adapted to understanding probabilities and the kind of logic involved, especially in this context that is so divorced from the every-day world.

2) Most people accept all too readily that they have no hope of understanding either what the experiments are trying to do or the technical details of what they really do.

3) Few realize that Bell's original, rigorous inequality is replaced in real experiments by substitutes involving additional assumptions. They are not equivalent to it or even necessarily to each other unless the assumptions are correct.

4) "Realists" can see at a glance that the subject has no relevance to the actual world!

5) They, therefore, have not studied the experiments and hence have not realized that they should have been demanding to see more information—more raw data and more results for different parameter settings.

6) Important contributors to the field, notably John Bell and Alain Aspect, have made mistakes, copied by others.[27]

7) The literature is voluminous and confusing. Very little is relevant to the actual experiments.

8) Theorists realize that quantum theory itself is at risk. Regardless of all the "conceptual difficulties," it is too "successful" to abandon without good experimental evidence.

9) "Quantum computing," etc. depends on quantum theory being right,[28] and the computing industry is currently an important source of funds. (It has so far tolerated the fact that nothing spectacular has been achieved. Hopefully the research will produce useful results—advances in optical computers, for example—even if the original idea is totally misguided.)

Conclusion

I hope I have told you enough to see that the scientific community has two choices: dismiss the whole idea of (non-local) quantum entanglement as obvious nonsense or carry out the tests more carefully, reporting them in a way that allows for a fair comparison between the rival theories.

If your own theory suggests that instantaneous action at a distance really happens, so be it. The Bell test experiments, however, give no support for this idea. At best, they could give only indirect evidence, but my investigations indicate that they deserve to be forgotten. No valid test has ever been done.

Acknowledgment

I should like to thank my friends at Aberystwyth and the many correspondents who have supported me in this work. It would not have been possible without the kind help of the University of Wales Aberystwyth, who, despite the fact that I am not affiliated with them, allow me the use of their library and computer.

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12. Perhaps to say that there is a claim that they influence each other is going a little beyond the facts. Strictly, what is claimed to have been demonstrated it that the *setting, a*, of one detector influences the probability of detection at the other.

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14. Spot the mistake in this diagram! A Stern-Gerlach setup in which the magnets were identical would not work: there has to be asymmetry otherwise when one is attracting so is the other, with equal strength, so the atom does not know which way to go. I inherited this mistake from Bohm. It does not affect the validity of my argument or his.

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On the Possible Role of Virtual Neutrons in Cold Fusion

Lino Daddi*

Introduction: p+e Fusion

Some cold fusion researchers[1,2,3] deem that in metal-hydrogen systems p+e fusion, by forming neutrons, may be a precursor of processes capable of producing ultimately the many new nuclides observed in several experiments. The basic mechanism would consist of neutron captures and possible subsequent β-decays. The experimental results look sometimes like simple increments of a unit of the mass number, as in the transmutations of ^{39}K into ^{40}Ca observed by Bush[4] and of ^{56}Fe into ^{57}Fe observed by Ohmori.[5] Other experiments[6,7] suggest multiple events.

Since 938.21 MeV is the mass of the proton, 939.50 MeV the mass of the neutron, and 0.511 MeV the mass of the electron, the reaction:

$$p + e \Rightarrow n + \nu \qquad (1)$$

is endoenergic, with Q = - 0.78 MeV. An electron with equal or higher energy has very little chance of undergoing the fusion with a proton (the cross section is evaluated as less than 10^{-20} barn).[8,9]

For an electron of kinetic energy less than 0.78 MeV, quantum theory contemplates[2] the possibility of a *virtual neutron* near a nucleus by which it could be captured, becoming a real neutron. Indeed, neutron capture is almost always strongly exoenergic, so it can overcompensate the lacking energy to the real neutron synthesis. From the partial reactions:

$$\begin{cases} p + e \Rightarrow n_{virt} + \nu \\ n_{virt} + {}^{A}N \Rightarrow {}^{A+1}N \end{cases}$$

the total reaction should be:

$$p + e + {}^{A}N \Rightarrow {}^{A+1}N + \nu \qquad (2)$$

which may also be written as ^{A}N (pe, ν) ^{A+1}N. It should remain very improbable (also being a three-body reaction).

When ^{A}N coincides with a proton (2) becomes one of the initial nuclear reactions for the hydrogen nuclear reaction considered in astrophysics:

$$p + p + e \Rightarrow d + \nu \qquad (3)$$

or also p(pe,ν)d. Its Q-value is 1.44 MeV (the difference between the binding energy of the deuteron and the threshold energy for the neutron synthesis). Dufour,[10] who considered the virtual neutron mechanism, admitted that nearly all the energy is carried away by the neutrino, while the deuteron carries only the recoil energy (about 1 KeV).

Chatterjee[2] reminds us that in the collapse of supernovae a high overlap of electrons and protons occurs at incredible densities, so that neutrons and neutrinos merge according to (1) and (3): a similar physics (nucleo-weak model for chemically-assisted nuclear transmutations) could be mimicked in particular situations as, for instance, in thin films of nickel and other hydrogen loaded metals, which can promote the formation of dense proton and electron plasmas.

The Borghi-Conte Experiment

a) The don Borghi Experiment

The first attempt to observe proton-electron fusions was devised by don Borghi,[11] whose work can be cited as a precursor of the researches aimed at producing nuclear reactions forbidden by current quantum mechanics.

In a metallic chamber structured as a klystron, a cold plasma of protons and electrons was maintained; the electromagnetic oscillations frequency was of the order of 10^{10} s^{-1}. The alternating microwave electric field produces, according to Borghi, a considerable number of collisions between the particles. The neutron detection was by activation detectors situated outside the walls of the chamber. Such a technique was found necessary due to the large noise from the microwave field, which disturbed the pulse counters. This is considered a good choice also, because the induced radioactivity assures that neutrons really are produced. Unfortunately, the experimenters were satisfied of having obtained a qualitative proof and failed to make use of expedients which, without complicating the measurements, would have shed more light upon the intensity of the neutron sources and upon the energy of the produced neutrons.

A lot of metals, having characteristics also very different, such as silver, gold, dysprosium, indium, thorium, and uranium, certainly were activated, or their activity was modified, without moderators interposed.

The induced radioactivity (from n,γ reactions) in the stable detectors was measured in low background conditions, with sufficient precision and reproducibility, but Borghi and co-workers decided not to utilize these measurements, not knowing what cross sections were to be used.

The writer has tried calculating the S intensity of the neutron source by utilizing the experimental data concerning the stable detectors with the thermal cross sections and by supposing a common detection efficiency; rather different values (from $2.5 \cdot 10^3$ to $1.3 \cdot 10^5$ n/s) resulted. This disagreement may be a confirmation of the hypothesis that it is a question of neutrons with significant epithermal components, but it may also be explained by strong differences in the detection efficiency. I think the silver activation obtained by Borghi at limit of sensitivity of his apparatus is very significant. By employing (with the necessary adaptations) the criteria discussed in an earlier paper[12] it should be considered as evidence of at least 10^4 n/s emission.

The use of uranium detectors was perhaps suggested to Borghi by the nearly constant capture cross section of ^{238}U,

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though this could be nullified by the many resonances. Moreover, the background was so high that the statistical errors on the net counts appear prohibitive, so that whatever calculation is made using those is hazardous. In the case of thorium, the background due to natural radioactivity, also without particular precautions, resulted in being less penalizing with respect to the uranium case. Unfortunately, the calculations reveal some shortcomings, which made this determination quantitatively unreliable.

Moreover, they could have looked for the possible radioactivation of the metallic walls of the chamber; in particular ^{64}Cu ($T_{1/2}$ = 13 h; E_γ = 1.35 MeV) by the (n,γ) reaction on the natural isotope ^{63}Cu, which could be produced in the brass parts.

In spite of the significant defects, particularly in the data analysis, there can be no doubt that Borghi obtained clear and reproducible radioactivations; therefore, it is presumed that in the cold plasma many neutrons were generated. The fact that they produce many reactions without having crossed any moderator suggests they are born with a very low energy, near the thermal values. This could exclude that they are due to fusions between nuclei of hydrogen isotopes, for instance between the few deuterons present in the plasma. One may assume they are produced by p+e synthesis through a virtual neutron mechanism. All the same, their numbers seems hardly compatible with the expected cross section of 10^{-20} barn.

b) The Conte Repetition

Recently Conte and Pieralice[13] repeated the Borghi experiment following the previous one in the cell structure and in the cold plasma production. Samples of Mn, Cu, and Au were activated. Suitable improvements were adopted, such as the replacement of the Ge γ-spectrometry with the NaI(Tl) one in induced radioactivity measurements; also the use of thermoluminescence dosimeters of the type LiB407. A neutron production of 10^5 n/s was found; unfortunately, few details are supplied about the experimental data and about their analysis.

Other Experiments

The two following experiments have in common the fact that fast electrons were made to impact on hydrogen-loaded aluminum. Al does not absorb large quantities of hydrogen, but it presents some advantages:

– the absorbed hydrogen diffusivity in Al is high.

– Al has a rather high electro-negativity, and thus there are many electrons available.

– Al presents quite a low absorption of the fast electrons (as those emitted by a beta source).

a) The Second Conte Experiment

Conte and Pieralice[9] performed a second experiment in which the encounter of the protons and of the electrons takes place inside an Al cathode where the electrons emitted by a ^{90}Sr-^{90}Y beta source of about 2.4 KBq impact the protons electrolytically concentrated (most electrons having energies less than 0.78 MeV when interacting with protons). Compared with the previous experiments the electron flux is very much smaller and the proton number is decidedly greater, so one can expect less neutron production (by several orders of magnitude). A BF_3 detector, connected to a multichannel analyzer, indicated a reproducible production of about 60 n/h (against a very much lower background), which allowed the experimenters to evaluate the reaction cross section as about 1 mb, a value which is more than 17 orders of magnitude higher than expected. According to Conte, this seems to point to the conclusion that neutrons could be formed by a proton-electron fusion different from the synthesis considered in ordinary quantum mechanics.

b) The Kamada Experiment

Kamada[14] performed experiments with hydrogen-implanted Al with a high near-surface loading. Following bombardment with an intense electron beam in the range 200-400 KeV, a production in CR-39 of traces of charged particles was observed (from almost the background level to more than four times that level) with energy greater than 0.2 MeV. The number of emitted particles is controlled both by the brightness of the electron beam and by the amount of implantation.

According to Kamada, the probable reaction is (3), which would present the quoted cross section of 10^{-20} barn. He indeed does his best to confirm this value by attributing to the reaction electrons the flux which one can deduce from an electron density of 10^{22} cm^{-3} and a velocity about ten times the Fermi velocity in Al. The electrons should be secondary electrons, produced by the primary ones through excitations of K-shell, L-shell, and conduction electrons.

The electron energy is below the threshold energy of 0.78 MeV, so it does require the virtual neutron hypothesis. Because the energy produced by the reaction (3) should be acquired almost exclusively by the neutrino, the observed particles couldn't be the deuterons of this reaction.

Moreover, the traces left by these particles on the detectors were not radially directed from the small bombarded area on the Al specimen, but they showed rather random directions.[15] Kamada has also worked with deuterium, getting similar results.

Interpretation

In the above experiments, it seems impossible that the electrons and the protons could achieve sufficient energies to produce real neutrons, except a fraction of the ^{90}Y beta rays; so the virtual neutron hypothesis seems necessary.

a) The Virtual Neutrons Hypothesis

As we have seen, in the neighborhood of a ^{A}N nucleus (which could belong to the neutron detector) the p+e interaction could form a virtual neutron, immediately absorbed by the same nucleus, according to (2).

Since it is unthinkable that virtual neutrons could be found very far outside the nuclear dimensions, a mechanism has to be thought up which transfers proton-electron squeezed couples to convenient distances where the two-body reaction can take place between the nucleus ^{A}N and the (p,e) couple turned into a virtual neutron.

An explanation should take into account the formation of hydrogen *"miniatoms."*

b) Formation of Miniatoms

Several authors[3,16-22] independently conceived processes of p+e (but also d+e), which are temporary associations consisting of compressed hydrogen atoms (hydrogen miniatoms), with the electron nearer the nucleus than the lowest Bohr radius, in new energy levels, even very deep ones. They could be formed under conditions which aren't the same in the various theories (not all requiring a crystal lattice); their formation could be accompanied by electromagnetic emissions.

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A very compressed miniatom should behave as a neutral particle in crossing into matter, so it could come nearer to nuclei, with which the proton could undergo processes of cold fusion.

Some of these theories seem very revolutionary[17,20,21] and are to be considered with great caution. Recently, experiments were described which seem to support the existence of quantum states below the Bohr fundamental level.[23] Other ones tend to remain in the framework of traditional quantum mechanics, to which they bring significant extension.[16,18] The author gathered the various proposals in a recent review,[24] to which the reader is referred for details.

Only some of these authors[3,17] considered the hypothesis that the miniatoms may decay by electron capture; however, the virtual neutron realization could be much more likely than models involving normal hydrogen.

c) The Complete Process

In short, the precursor mechanism may consist of the formation of perhaps very unstable, miniatoms able to cross material thicknesses, nearly as if they were neutrons, and subsequently to react with nuclei like virtual neutrons. The probability of (2) could be determined by the cross sections for the capture of the virtual neutrons and by the efficiency of the intermediate process, so depending on the particular mechanism (among these proposed in Reference 3 and from 16 to 22) which is working. According to the experiments, it ought to be a greater probability than without miniatoms formation, because now the process is a two-body reaction; impurities of the materials may also undergo observable transmutations. For instance, in the Borghi-Conte experiment the neutrons could not form from (1) in the cold plasma, but a reaction of the type (2) could happen in the walls of the cell or at once on the neutron detectors. A weakening (corresponding to 0.78 MeV) of the capture γ-ray spectra could be a confirmation of this hypothesis.

The double event "miniatom formation" and "compound nucleus formation" could also justify the essential features of the Kamada experiment where, instead of the (n,γ) reaction, there might occur a reaction of the (n,α) type with $Q>0.78$ MeV with nuclides present (also in little quantity) in aluminum or in CR-39. The random direction of the charged particles suggests that the neutron source was different from the narrow zone of the electron's incidence.

The origin of the X-rays observed by Kamada[25] and in other experiments could be connected with the formation of miniatoms.

Alternative Theories on p+e Fusion

Some theories, partially or fully alternative to quantum mechanics, might be able to explain the main anomalous features of the experiments cited. Here they are briefly summarized.

Borghi[26] himself paid particular attention to the frontier between the validity regions of the nuclear and atomic laws, considering that for distances less than the Bohr radius, but greater than the proton dimension, both laws must furnish the same description (principle of correspondence). Therefore the p+e system can appear either as a hydrogen atom or as a neutron. Borghi was thinking that the atom is formed by a continuous interaction, whereas by a discontinuous interaction a system is formed which has all the characteristics of a neutron.

The p+e synthesis finds different interpretations in the theories of Santilli[27,28] and of Conte,[29] which consider inadequate the ordinary quantum mechanics approximation of treating the two particles as point-like. They are imagined to form the neutron while keeping their identity, by analogy with the old Rutherford model. The greater rest mass compared to the rest mass of the component charged particles is determined by the different light speed, which makes the electron mass increase through a relativistic effect in the proton space-time.

After the first work of 1978, Santilli developed the hadronic mechanics[28] of which a synthesis up-to-date was published in a recent series of papers in *Infinite Energy*. The isoequivalence principle involves the rest mass of the electron becoming 1.36 MeV inside the medium of the proton.

Thanks to the contact forces, the center can be occupied by the electron. The model of Santilli produces a precise and invariant representation of all characteristics of the neutron, either relativistic[30] or non-relativistic,[28] and foresees the stimulated decay by γ-rays of suitable energy.

The biquaternion quantum mechanics, proposed subsequently by Conte,[27] has many things common to hadronic mechanics, with various differentiations. The Clifford algebra gives a suitable structure for a unified treatment of the relativistic fields and of hidden parameters. In particular, Conte succeeded in proving the quark structure.

Since 1960[31] Phipps has been very critical about some basic aspects of quantum mechanics, including the Heisenberg postulate. Today[3] he asserts that an electron can be connected to a proton (free or bound in a nucleus) in states shrunken in size to the order of some multiple of the electron Compton wavelength.

From such states, by further contraction up to the order of the classical electron radius, it could form a neutron (free or bound in the nucleus). The identities of the single particles should be kept, but the electron, in the neutron or in the nucleus, should not be directly observable, by having imaginary momentum (with real energy). The results of the second experiment of Conte and Pieralice might prove this possibility, according to Phipps. No mechanism is indicated for which the rest-mass increases by 0.78 MeV; instead the contraction up to the electron Compton wavelength could produce an energy variation of few KeV, so it seems inadequate to facilitate nuclear transmutations by means of the virtual neutron mechanism.

d + e Reaction

With deuterium, electron capture is still more endoenergic ($Q \cong -3.1$ MeV). At low energies a mechanism involving virtual neutrons may be supposed,[32] according to the reaction:

$$d + e \Rightarrow n_{virt} + n + \nu \qquad (1')$$

where the second neutron is available only when the first has become real.

To that hypothesis it was easy to contest,[33] also discounting the very small value of the electron capture probability, that the virtual neutrons are "off-shell" by a few MeV (so their range cannot be greater than a few tens of fm). If the encounter takes place in the immediate closeness of a nucleus of mass number A, the total reaction may be:

$$d + e + {}^{A}N \Rightarrow {}^{A+1}N + n + \nu \qquad (2')$$

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Equation (2′) is made exoenergic by the prevailing neutron binding energy and the n_{virt} could be absorbed without the classic mechanism of the previous moderation.

The precursor miniatoms hypothesis can be applied to the deuteron-electron reaction, with a strong increase of the probability of the reaction, which becomes a two-body reaction between the deuterium miniatom and the ^{A}N nucleus.

The absorption of the virtual neutron may justify those transmutations, observed in some deuterium cold fusion experiments, in which the mass number increases by one unit.

In the recent past, in metal-deuterium systems, isotopic shifts were observed in which the mass numbers were increased by one unit. I cite the ^{6}Li to ^{7}Li transition, as observed by Coupland[34] in the palladium used by Fleischmann and Pons, and the ^{53}Cr to ^{54}Cr transition observed by Mizuno.[1]

Often the atomic number underwent equal increment by beta decay, as in the ^{3}He production by Arata,[35] following interaction between hydrogen and deuterium. One can suppose the reaction:

$$d + e + p \Rightarrow {}^{3}H + n + \nu \qquad (2'')$$

with the subsequent decay of tritium to ^{3}He.

If the ^{A}N nucleus also absorbs the second neutron in a single interaction, it would be:

$$d + e + {}^{A}N \Rightarrow {}^{A+2}N + \nu \qquad (4)$$

This hypothesis could replace all the "dineutron" of past formulated theories.[23,36,37] It could also explain the ^{55}Mn to ^{57}Fe transmutation observed in biological heavy water cultures.[38]

In deuterium we would have:

$$d + e + d \Rightarrow {}^{4}H + \nu \qquad (4')$$

with the decay of ^{4}H in ^{4}He.

Karabut[39] conducted some experiments with deuterium which offer many analogies with the Borghi-Conte experiment. It is a matter of low voltage electric discharge at a low deuterium pressure, but without the superimposition of the h.f. electromagnetic field. The main purpose of those experiments was the excess heat production, but some researches aimed at the identification of the nuclear emissions were also made.

Neutron production of almost 10^2 n/s was observed by various methods, including silver foil activation. Alpha particles are found in quantities 3 to 4 orders short of those needed to explain the excess heat. Also, there might be a reaction of (n,α) type of the virtual neutrons with nuclides present (also as impurities) in the neighboring materials. For example, $^{6}Li(n,\alpha)^{3}H$ or $^{10}B(n,\alpha)^{7}Li$.

Conclusions

The virtual neutron mechanism would be able to explain by itself some cold fusion experiments in which the observed effects can be attributed to neutrons produced by the p+e or d+e synthesis, and they seem generated as slow neutrons. But when the total cross section appears very much greater than the expected one, a more complex mechanism has to be devised, like that of miniatoms. In fact, miniatom formation makes the proton stay very near to the electron for a relatively long time, with great increase in the probability of the observed reactions, which would be determined by the neutron capture cross sections and by the efficiency of the miniatom formation. On the whole, it would be very much greater than without miniatom formation, also because the process is now a two-body reaction.

This mechanism could allow the transfer of the neutron source to a significant distance from the site where hydrogen is concentrated,[15] that is, where the encounter between protons and electrons takes place.

The poor reproducibility in many cold fusion experiments could be due to irregularities in miniatom formation and to their instability; an analogous hypothesis was put forward by Phipps.[3]

For electrons, whose energy is above the threshold, as those of the hardest fraction of ^{90}Y beta ray spectrum in the second Conte experiment, the virtual neutron-miniatoms hypothesis would not be necessary, but a direct p+e fusion seems incompatible with the very much greater measured cross section. A repetition of this experiment by employing beta sources of end point less than 0.78 MeV could clear up the possible role of virtual neutrons.

Also, for deuterium one may suppose the temporary creation of miniatoms, especially when the observed reaction rate exceeds all classical expectations.

The Borghi-Conte, Vysostskii, and Karabut experiments were conducted without metal loading by hydrogen or deuterium; this would indicate that a crystal lattice isn't necessary for the production of low-energy anomalous nuclear reactions.

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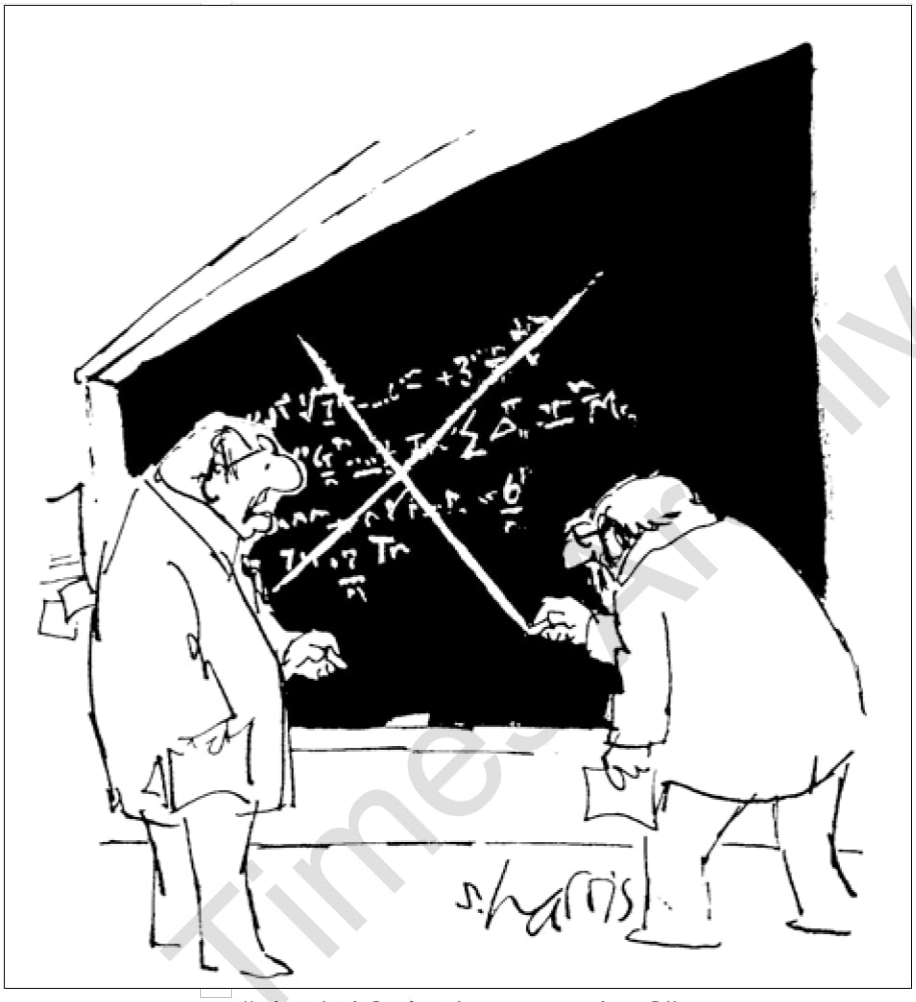

"That's *it?* That's peer review?"

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No persons are more frequently wrong than those who will not admit they are wrong.—Rochefoucauld

Submitted by Walter S. Rosenthal

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Boson Condensation in High Temperature Systems and Its Relation to Cold Fusion

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Introduction

In the field of radiation-less fusion, commonly called "cold fusion," the production of excess heat initially reported by Pons and Fleischmann[1] is now being routinely observed. The status in 1995 has been summarized by Storms.[2] Recently, he has, in fact, published a recipe[3] for assuring that any researcher can reproduce his work. It involves palladium disks and a LiOD solution. McKubre *et al.*[4] also have a recipe for doing the same, although the details differ. Miles *et al.*[5] have shown that both excess heat and ^{4}He are produced simultaneously and concomitantly. Hence there can no longer remain any serious doubts about the reality of the phenomenon. The quantity of excess heat varies 40 to 100% depending on the worker. This quantity of heat is significant but not large. Doubtless improvements will now continue to be made.

The significant hurdle to utilizing this breakthrough is related to the Carnot Cycle. That is, the production of heat at room temperature thermodynamically can only be effectively used by having a lower temperature region available. Refrigeration is about 100 years old by now and the methods are well-known. But just like superconductivity at liquid nitrogen temperatures, "cold fusion" is economically not very interesting at this time because of this limitation.

What is needed is a method of producing similar or larger quantities of excess heat at 200° or higher, such as with steam power. This goal is not currently as available as the Pons-Fleischmann method currently delineated by Storms or McKubre, since this is the only accepted way. However, the situation is not hopeless.

Biberian[6] in France worked with the perovskite $LaAlO_3$ which is a good proton conductor, in the temperature range of 400°C to 600°C and produced 10 times the input energy. He suggested that the reaction of the deuterium atoms yielded ^{4}He, which he now proposes to verify in subsequent experiments. In Japan, Mizuno *et al.*[7] had similarly shown large quantities of excess heat with another perovskite, $SrCeO_3$.

In Russia, there were a number of interesting reports. Samgin *et al.*[8] carried out electrolysis of deuterated $SrCeO_3$ in the temperature range of 200°C to 750°C and produced small amounts of excess heat. They also checked for nuclear reaction products.[9] Vakarin *et al.*[10,11] noted that a high degree of perfection of the single crystals was required for successful heat production. The neutron emission stopped after five minutes. Polishing of the single crystals failed to produce useful results.

Kuznetsov *et al.*[12,13] found anomalous heat effects with the deuterated KD_2PO_4 but not with the equivalent KH_2PO_4. The Curie point may have been involved. Kaliev and Filimonov[14] observed the release of neutrons after loading a ceramic with deuterium for about five minutes. Filimonov[15] used a Pons-Fleischmann-like electrolysis in the temperature range 85°C to 100°C but with several other metals. The dislocation content was 5×10^{-4} to 10^{-9} per cm^2 at the end of the experiments. Their two preliminary papers were not published in the proceedings.

All of these experiments are interesting but are not in themselves conclusive. They do suggest that something other than palladium (such as various perovskites or other proton-conducting solids such as zeolites) may also produce the necessary nuclear reactions with D + D being favored. There is evidence that the experimental temperature may indeed be raised above room temperature and this will improve the thermodynamic efficiency.

The essential breakthrough involves a combination of solid state physics and material science. The real solution requires a reevaluation, and in a sense, an amplification/completion of our existing theories of solid state matter. The concentrated efforts to obtain ways around, *i.e.* to avoid Gamov barrier penetration, are in all probability now misdirected. The real insight will come, in this author's opinion, from a proper attention to solid state physics rather than to particle physics and the Gamov problem.

Previous Theoretical Papers

The present author has written several papers[16,17] which deal with the relation between cold fusion and Bose-Einstein Condensation of deuterons. It was first outlined by Chubb and Chubb,[18-20] who advanced the idea that the phenomenon was really a solid state problem and should not be treated as if two bare particles were colliding in a vacuum. Their studies were couched in an Ion Band States description. These articles pointed out that deuterons were bosons. The second article by Waber involved a modification of the Fujita-Watanabe theory of superconductivity. It pointed out that all of the requirements of that theory were met. Thus, bosons in a sea of fermions was a legitimate state and the pairing of the bosonic deuterons to form ^{4}He was a reasonable mechanism.

Bose-Einstein Condensation

These ideas are now recast in a more modern description. To really treat the phenomenon adequately, we will review the current state of superconductivity and the relevant thermodynamic situation.

The phenomenon of Bose-Einstein Condensation (BEC) has been mentioned by some of the authors cited above without much detailed information being given. BEC also is not well-described in many of the various textbooks, except in the case of the superfluidity of helium. However, there is a new reason for the recent publication of serious works in this field. It deals with the superconductivity of high T_c

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materials. The well-established Bardeen-Cooper-Schriefer (BCS) theory is not quite adequate to account for these new materials and their properties.

The existence of metal-oxide materials which are known to superconduct at half the room temperature (135°K) indicates that room temperature superconductivity may be possible. The established theory of superconductivity, the BCS theory from 1957, is incomplete. It only describes a metallic Fermi-liquid form, up to about 23°K.

A new thermodynamic theory has emerged which describes the new high-temperature oxide superconductors. It embraces a cross-over between the conventional BCS description and the theory of Bose-Einstein condensation. These are extremes of behavior. The criteria for high temperature superconductivity is that there are high-density regions and the possible existence of local composite bosons coupled tightly to high energy Fermi-pair excitations. In short, it is a mixed region between BCS and BEC with bosons in a Fermi sea.

High temperature superconductivity occurs in strongly-coupled, highly-correlated metal-oxide materials with extremely short coherence lengths. They may even undergo Mott metallic-insulator transitions.

BEC is covered in two books, one by Blatt[21] and a recent compilation volume by Griffin, Snoke, and Stringari.[22] The principal difference from the BCS theory is that high temperature superconductivity relies on the Ogg-Schafroth theory.

I. Historical Note

The history of the idea of Bose-condensed electron pairs is not untypical of the history of new ideas in physics,[23] particularly when new concepts are being advanced. The idea that Bose-Einstein condensation might be responsible for superconductivity dates back to Ogg.[24] Working with a sodium-liquid ammonia solution, superconductivity at 70°K was observed in 1946. He had indirect evidence of Bose-Einstein condensation. He performed the analogue of the classical Kammerlingh-Onnes experiment. Namely, the solution was prepared in a thin disk shaped vessel and plunged into liquid air between the poles of an electromagnet. When the magnet was turned off, a persistent current occurred at 90°K as detected by a sensitive magnetometer. He reported that this current remained even on warming the vessel up to roughly 180°K. He explained that polarization more than compensated for their electrostatic repulsion.[25,26] Boorse *et al*.[27] found that it was difficult experimentally to reproduce the phenomenon.

Since Ogg was an experimental chemist and the known metallic superconductors at that time did not exist above 20°K, his work was strongly discounted and he was personally scorned. Eight years later, the Australian theorist Schafroth[28] applied the concept to the anomalous behavior of $SrTiO_3$. It too was ignored for several more years in spite of using the theorist's language. Paired electrons in 1956 were not yet a respectable idea.

The following is a paraphrase of a quotation by Blatt:[29] It is interesting that one of the coincidences which are surprisingly common in the history of Science, L. Cooper and Schafroth were at the Princeton Institute of Advanced Study at the same time and they shared ideas, but the treatment of coupled electrons to form bosons occurred to each of them independently and without recall of their discussions.

II. Current Situation

The theory is now known as the Ogg-Schafroth theory of superconductivity. Eagles[30] considered the mixed BCS/Bose-Einstein situation. Alexandrov and Raninnger[31,32] developed the bipolaron theory, which lead Bednorz and Möller to search for cuprate superconductors. The current situation is reviewed by Alexandrov and Mott.[33] Nozieres and Schmitt-Rink[34] established the continuity between the BCS and the Bose-Einstein regimes. Currently there are a large number of specialized models and extensions of these concepts, which will only distract the reader from the essential points being marshalled here.

First, we will describe the high density extreme of Bose-Einstein condensation.

III. Behavior of an Ideal Bose Gas

At a non-zero temperature, an ideal Bose gas yields a transition temperature, which is

$$T_c = (2\pi\hbar^2/M)(N/2.16\,V)^{2/3}$$

With reasonable values of M, the mass per particle, N, the number of particles, and V, the volume of the solid, the transition temperature T_c, is of the order of 10^4 K or comparable to the Fermi degeneracy temperature of a metal. This, however, is misleading. The dilemma may be lifted by discussing the degeneracy temperature in greater detail. While the behavior of an ideal Bose gas in its condensed state has many properties in common with superconductors and superfluid helium, the correspondence is not perfect.

Here the "bosons" are really composite bodies of some sort. Schafroth took up the Bose-Einstein model and regarded "free electrons" in a solid as clearly unsuitable candidates for superconductivity. Another type of composite body is an exciton, which is a bound state of a conduction electron and a positively charged hole in the conduction band. (Both are fermions.) As such, this combination resembles positronium and is electrically neutral. A better kind of composite body would be a pair of electrons. While such Cooper pairs[35] are present in the BCS theory, we distinguish electron-pairs in the Bose-Einstein condensed state as Schafroth pairs. An article in *Scientific American* discusses composite bosons (dressed with magnetic flux lines) in connection with the idea of Fractional Quantum Hall Effect[35] and its connection with superconductivity. They describe the composite bosons involved as paired Fermion holes.

IV. Schafroth Condensation of Electron Pairs

Schafroth pairs are by no means the same as elementary Bose-Einstein particles. We require only the existence of a type of condensation similar to Bose-Einstein condensation for superconductivity. Fermi statistics of electrons *does not* prevent us from putting many electron pairs into a single one-pair state $\phi(\mathbf{k}_1, \mathbf{k}_2)$ and the Pauli exclusion principle is not a hindrance to the condensation of electron pairs but it is actually a positive influence.

The wave function can be written as

$$\psi(1,2,3,4) = \phi(1,2)\phi(3,4) - \phi(1,3)\phi(2,4) + \phi(1,4)\phi(2,3)$$

A calculation shows that this function is antisymmetric with respect to any exchange of particles and thus is permissible. While both electrons are in state ϕ, the expectation values may differ. It is useful to treat the following special case as an example of a more general one. To this end we apply the fol-

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lowing operator υ_{12}

$$\upsilon_{12} = \delta(r_1 - \rho_2)\ \delta(r_2 - \rho_2)$$

to the expectation value

$$\int \psi^*(1,2,3,4)\ \upsilon_{12}\ \psi(1,2,3,4) d^3r_1 d^3r_2 d^3r_3 d^3r_4 = [\psi, \upsilon_{12}\psi]$$

That represents the probability over ψ of finding particle 1 at position ρ_1 and particle 2 at position ρ_2 and the other two particles anywhere else. Since ψ is anti-symmetric, this probability is *symmetric* under the exchange of particles 1 and 2. This probability is not equal to the absolute square of the basic "pair" wave function $\phi(\rho_1, \rho_2)$; thus it is *not* a two-particle projection of the four-particle wave function ψ even though ψ is "built up from" the factors ϕ. We also distinguish it from a pair of single particle-states.

The single particle-states $\mathbf{k}_1$ and $\mathbf{k}_2$ are occupied, each with an occupation number of 1. Hence there is no room for other particles. However, we put the pair of particles into a physically reasonable two-particle state $\phi(\mathbf{k}_1, \mathbf{k}_2)$, then the probability of finding one member of the pair in a "single-particle" state $\mathbf{k}'_1$ and the other member in $\mathbf{k}'_2$ is given by $|\phi(\mathbf{k}'_1, \mathbf{k}'_2)|^2$ and this occupies an infinitely small part of the phase space for any reasonable pair wave function.

In the general case, this probability is of the order of $(1/V^2)$, in the limit that V, the volume, approaches infinity. However, there is ample room for putting more pairs into the same pair wave function. In short, the Schafroth condensation of pairs corresponds, in this formalism, to a "separable" dynamical pair correlation matrix.

Introducing an auxiliary function

$$v_0 \equiv v_0 \exp\ (\ 2ß\mu)$$

and examining the grand canonical potential Ω' (T, V, μ) of the pair for various values of v_0. The exponential factor contains the chemical potential μ of the pairs and ß is the reciprocal of $\mathbf{k}_B$T.

If we think of our pairs as Bose-Einstein particles with an energy spectrum of n_α, the condition for the "no overlap" approximation to hold is that μ_2 be large and negative. The extreme case of Bose-Einstein condensation is achieved when μ_2 moves up to the lowest pair energy $^{n}{}_0$, when $\nu = 1$. This can never be achieved if all the υ_α are negative. This situation would correspond to the pair correlations being repulsive.

The simplest case where the grand canonical potential can be evaluated is the "no overlap" approximation which is due to Schafroth

$$\Omega' = +k_B T \ell n (1 - \nu_\alpha)$$

and the average number of electron pair states

$$<\upsilon_\alpha> = \upsilon_\alpha/(1 - \nu_\alpha)$$

is in agreement with $\in = +1$, in contrast to $\in = -1$ for Fermi-Dirac statistics.

There is at least one problem, namely the overlap integrals can *not* be ignored or even regarded as very small, once there is condensation. It is not surprising that the results of the calculations Ω' are different when $\upsilon_0 \sim 1$. For attractive correlations, we get an inverted Hund's rule which is applicable to light nuclei.

For $\nu_0 > 1$, the one pair-state ϕ accommodates a number of pairs, which is proportional to the volume V and makes a significant contribution to the free energy. Fermi-Dirac statistics do not prevent an infinite occupation of an electron-pair state.

Exclusion effects are minimized and energy is saved by putting all the pairs into the same state. The exclusion principle not only does not forbid it, but positively favors Schafroth condensation of fermion pairs.

IV. Coherence Length

The Pippard coherence length ξ is the distance between the two members of the same pair with opposite spins. It is the size of the pair, *i.e.* the range of the wave function for the relative motion of the pair. This quantity ξ is of the order of $1/k_F \sim 10^{-8}$ cm or 1 Å in Schafroth condensation, whereas it is of the order of 1,000 Å in the BCS theory. The correlation is relatively weak numerically but is important for the physical properties of superconductivity.

V. Prediction of the Condensation Temperature

So far, we have contrasted two extreme regimes, namely (a) the BCS theory of cooperative Cooper pairing of high-density, weakly-coupled wideband, essentially metallic electrons with (b) the Bose-Einstein condensation of low-density, strongly-coupled, narrow-band, isolated bipolarons, a second theory of superconductivity. That is, it is distinct from BCS. The contrast between the BCS and the BEC theory is exemplified by the Uemara plot presented as Figure 1.[36,37] On the left hand side of this graph, three critical temperatures, the logarithms of T_F the Fermi temperature, T_B the Bose temperature, and T_c the usual BCS critical temperature. These were plotted by Uemara *et al.* on the basis of their muon spin relaxation μSR experiments, conducted on oxide superconductors. Apparently, the right hand side was added by Elifritz.[38,39] There, the eSR experiments on metallic superconductors are also summarized. The abscissa scale is the renormalized particle density, name-

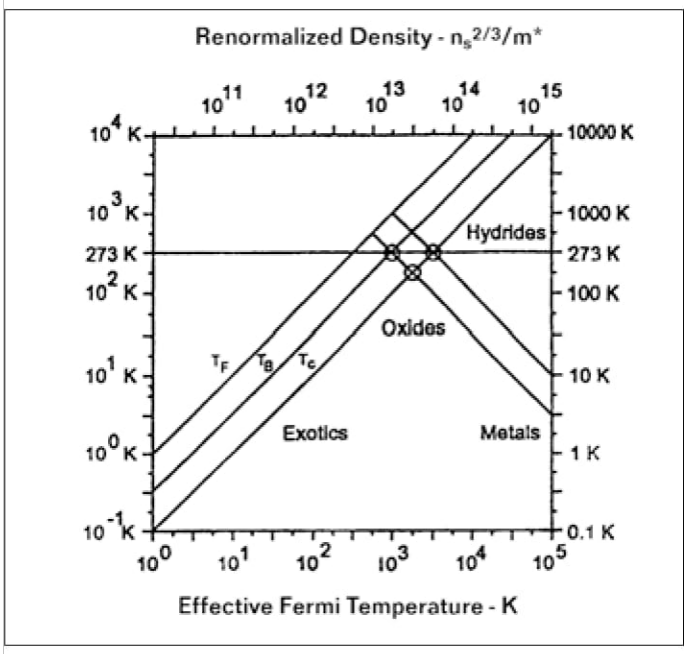

Figure 1. Uemura plot of the critical temperature as a function of the renormalized density. The temperature T_F is the Fermi temperature, T_B Bose temperature, and T_c is the usual BCS critical temperature. The lines on the left represent the muon spin relaxation data, whereas those on the right are for the electron spin relaxation data.

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ly $n^{2/3}/m^*$. The cross-over region is clearly indicated. *Note that in the middle mixed region, one finds hydrides.*

Various hydrides are predicted by this graph to exhibit superconductivity at melting-ice temperature, (*i.e.* 0°C). Before further discussion of the graph, an even higher temperature has now been reported by Shabectnik *et al.*[40] A cupro-selenide, $YBa_2Cu_3Se_7$ exhibits superconductivity at *boiling water temperatures, i.e. at* 371°K. So far the results have not been confirmed independently.

In order to describe the cross-over between weak-coupling BCS and strongly-coupling bipolaronic superconductivity in a electron-phonon coupled system, a boson-fermion model developed by the Grenoble group[41,42] is useful. It consists of a mixture of itinerant electrons and tightly-bound electron pairs. It is a natural extension of the bipolaronic model where the only charge carriers are tightly bound electron pairs. The Bose-Einstein condensation temperature can be estimated by

$$k_b T_B = 3.3 h^2 n_B^{2/3} / m_B \ .$$

Using reasonable values of the boson density (n_B. ~ 10^{22}/cm^3) and the boson mass (m_B about 10 electron masses), T_c can easily be of the order of a few hundred K. This makes this model attractive.

VI. Raninnger's Extension

Raninnger, Micnas, and Robaszkiewicz[43] have discussed the superconducting properties of a mixture of wide band electrons and real space pairs of electrons coming from a very narrow band. The local pair formation may be qualitatively described by bipolaron formation and the local pairs may exist as either on site or intersite pairs. The chemical mechanism as enhanced dynamic charge disproportion of the Cu-O (or Cu-Se) ligands in a highly polarizable dielectric medium. The short range attractive coupling between the electrons belong to a narrow band subsystem. The formalism for real space pairs was developed by Alexandrov *et al.*[44] and Robaszkiewciz *et al.*[45] They derived such a picture from an Anderson lattice with strong on site coupling. They rely on an operator $d_{i\sigma}^{(+)}$ of the localized electrons (which only exists in the form of bound states). Here $c_{j\sigma}^{(+)}$ and $c_{k\sigma}^{(+)}$ pertain to the wide band electron subsystem.

The situation is indicated by Figure 2 due to Emin.[46] Self-trapping of the carriers becomes energetically stable when the binding energy of the pair exceeds the strain energy of producing the atomic displacements. The strain lowering of the energy of two self-trapped carriers is enhanced when the two carriers reside at the same location, *i.e.* coalescence of the two carriers into a bosonic pair, shown on the right hand side of the figure. The formation of a singlet bi-polaron is roughly analogous to the formation of a covalent bond.

The bipolaron theory involves strong interaction between charge carriers and local lattice deformation. The following examples may be cited:

1) the $Cu^+(1) \Longleftrightarrow Cu^{++}(1)$ fluctuations accompanied by dumbbell $\Leftrightarrow$ square configurations of the surrounding oxygens of the chains in $YBa_2Cu_3O_{6+x}$.

2) the $Bi^{+3} \Longleftrightarrow Bi^{+5}$ fluctuations accompanied by the octahedral $\Longleftrightarrow$ tetrahedral configurations of the oxygens in Ba $(Bi_x\ Pb_{1-x})\ O_3$ compounds.

3) the $C_{60}^- \Longleftrightarrow C_{60}$ fluctuations in fulrenes accompanied by the tangential pentagon deformations, fluctuating between two extreme tendencies of the sp^2 configurations typical of graphite and the sp^3 configurations typical of diamond.

The strongly coupled dynamical fluctuations of the anion valency and of the atomic positions of the ligand environment show up as polaronic features.

They calculate the hopping integral of the local pairs of *d* electrons and the hybridization induced charge transfer between the *c* conduction and the *d* electrons which describe the fluctuating charge in the ligands. Depending on the integral $J_{ij} \neq 0$ (while $I = 0$ represents the direct and hybridization), the local pairs will condense into a superfluid state while the *c* electrons will remain in the normal state. The wide band electrons will screen effectively the long range Coulomb interaction of the charged local pair (LP), favoring local pair superconductivity.

If the $I \neq 0$, neither of the subsystems can become superconductive by themselves. It can be induced by the charge exchange proportional to *I*.

Without going into further detail, we can distinguish three physical situations as depicted in Figure 3, which depend on the relative concentrations of *c* and *d* electrons. We define Δ as the LP level from the bottom of the wide band. The situation is illustrated in Figure 3 due to Raninniger *et al.*[47]

> if $\Delta_0 < 0$, all of the *c* electrons are available in the filled conduction band and the *d* electrons form local pairs of electrons, *i.e.* LP lies well below the band of itinerant electrons.

> if $\Delta_0 > 0$, all of the *c* electron band is filled up to the Fermi level μ and the remaining electrons are in the form of a narrow band of *d* pairs slightly above μ, *i.e.* LP is very close to the Fermi level.

> if $\Delta_0 > 0$, such that the Fermi level $\mu < \Delta_0$, consequently all the available electrons occupy the *c* states, *i.e.* LP is well above the occupied energy range.

The finite temperature phase diagram presented in Figure 3 is plotted as a function of Δ_0/D where *D* is the width of the conduction electron band. On the left hand side or negative values of the parameter, (LPN) corresponds to the non-metallic first case of only localized electron pairs. (LPS + ES) corresponds to the second case or the superconductivity state involving both local pairs and wide band conduction electrons and <EM> indicates a normal

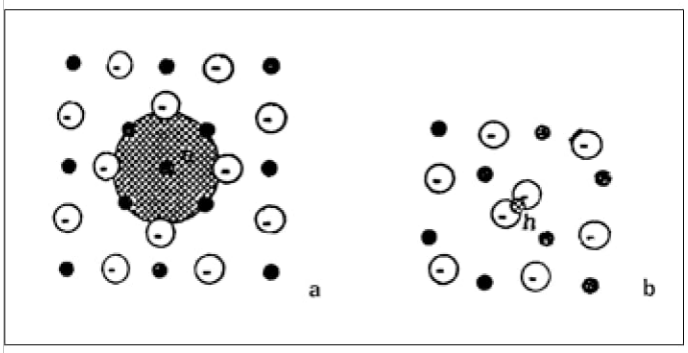

Figure 2. Large (a) and small (b) bi-polarons in an ionic solid (after Emin). The strain lowering of the energy of two non-interacting self-trapped carriers is increased when both carriers reside at the same location. However, the requirement that more than two electrons can occupy the same state mitigates against the formation of more than a pair.

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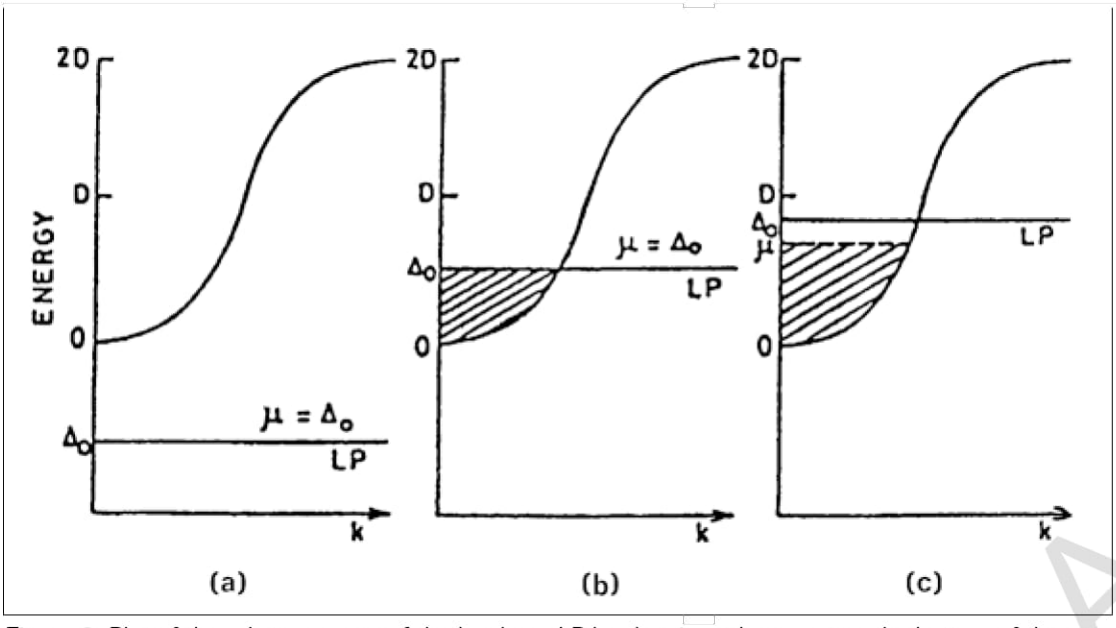

Figure 3. Plot of the relative energy of the local-pair LP level at Δ_0 with respect to the bottom of the conduction band in the absence of any interaction. Here μ represents the chemical potential. On the left hand portion, only the ground state is occupied. On the right hand portion, only the single-particle states of the wide-band conduction electrons are occupied. In the middle, both the LP and the single-particle states of the wide band are occupied. (After Reference 41)

metallic state of wide-band electrons.

Localized tightly bound small polarons were a conjecture made by Anderson[48] a number of years ago to account for the anomalous thermodynamic behavior. Systems with randomly distributed "negative Hubbard *U*" centers imbedded in a metallic matrix with double charge fluctuations between the centers were studied with the aim of increasing T_c. In the BCS type of superconductivity, the "negative Hubbard *U*" play the role of phonons. The idea was introduced by Raninnger and independently by Ionov.[49] Theoretical work made it clear that in systems with the electrons coupled to local vibrational modes, a metastable state between bi-polarons and itinerant electrons could be achieved when the coupling is of moderate strength.[50] The attraction between electrons in such impurity centers was considered to be due to the large polarizability of the surrounding medium ". . .together with internal correlations in the two-electron wave functions similar to He atoms or H molecules."[51] Dynamical charge fluctuations between adjacent units are required to obtain $U < 0$ effective impurity centers.

Using a meanfield approach, the Hamiltonian becomes

$$H^{MF} = \Sigma_{k\sigma} \in_k C^+_{k\sigma} + (\Delta_B - 2\mu)\Sigma_i b^+_i b_i + \nu\rho^{1/2}\Sigma_k (C^+_{k\uparrow} C^+_{k\downarrow} + C_{-k\downarrow} C_{k\uparrow}) + \nu x \Sigma_i (b^+_i + b_i) - \rho\nu x \qquad (5)$$

where the *C*'s represent electrons and the *b*'s represent bosons and where $\in_k = t[z - \Sigma_\delta \exp(ik\delta)] - \mu$. By diagonalizing (5) we obtain an excitation spectrum

$$\omega = \pm \sqrt{[\in k^2 + (\rho\nu)^2/4]}$$

for the electrons and

$$\Delta = \pm \tfrac{1}{2} \sqrt{[(\Delta_b - 2\mu)^2 + 4(\nu x)^2]} + 1/2\,(\Delta_b - 2\mu)$$

for the bosons.

Using a better approximation, RPA, the two level boson spectrum is replaced by a collective mode which is either phonon-like (in the absence of a long range Coulomb interaction) or plasma-like (in its presence).

One can derive the number of bosons n_B and fermions n_F. These equations (not given here) remain correct in the absence or presence of long range Coulomb interactions since the Thouless Criterion for superconductivity instability does not involve Coulomb interactions in the lowest order (neglecting vertex corrections). The quantity n_{tot} is equal to n_F plus $2n_B$.

We will summarize with the phase diagram of a local pair system as presented in Figure 4, where n_{tot} is replaced by n the concentration and the ordinate quantity is $\kappa_B T_c/J_0$ [where T_c is the critical temperature and J_0]. In this figure *SS* denotes the homogeneous superconductive phase, analogous to ^{4}He II. The region M represents a mixed phase where superconductivity and charge order coexist. The region CO is the charge ordered and NO is the phase with non-ordered local pairs. The maximum value of T_c occurs at MP, the multicritical point. It is attained at the transition to the charge ordered state when the concentration of bosons becomes large enough to cause repulsive action between them. T_c goes through a maximum as a function of the total number of charge but goes above the BCS value as the number of

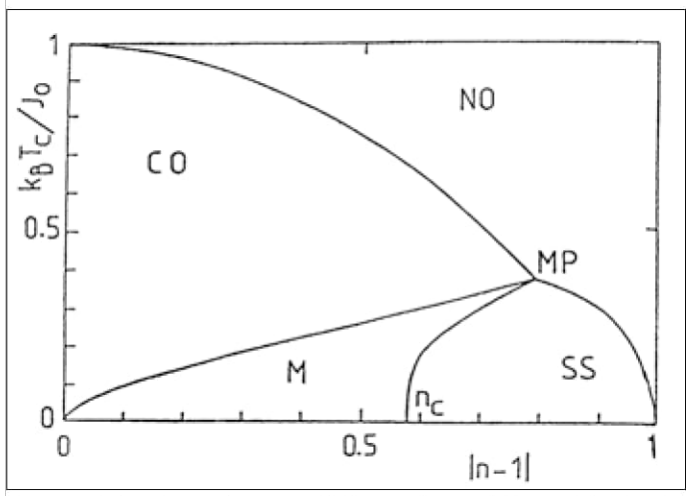

Figure 4. A phase diagram of the regions of the LP local pair organization as a function of the carrier concentration. Note that the pairing in the SS region is analogous. Raninnger *et al.* notes to that in ^{4}He II. Regions CO and NO correspond to presence and absence of ordering, respectively, of the charges, whereas region M indicates where a mixture of superconductivity and charge order coexists.

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local pairs increases. The coherence length, *inter alia* will vary continuously, in the mixed region, from that characteristic of local pair superconductivity towards that of the BCS theory.

Relation to Cold Fusion

We have shown that a very short coherence length of electron pairs and high transition temperature are important features of the mixed boson-fermion type of superconductivity in a variety of systems. The composite boson pair of fermions is itinerant.

In the palladium-deuterium system, we do not have composite boson of the type described above, but we do have a mixture of itinerant electrons and deuterons which are themselves bosons. It is proposed that there is a similar type of charge fluctuation, namely

$$Pd^0(d^9s^1) + D \Longrightarrow Pd^{-1}(d^{10}s^1) + D^{+1}.$$

This is due to the high electron affinity of palladium. Note here that d^{+1}, or deuteron, is a boson and the two deuterons may easily pair up.

Equation (5) does not distinguish the type of boson involved in the exchange between the electrons and bosons. One notes that the pairing results in a correlation which is similar to that found in the He atom. Thus the cusp condition relating to the proximity of the electrons in the Ion Band Picture by Chubb and Chubb remains pertinent here.

The Pippard coherence length ξ of a Schafroth pair (which forms a boson) is inversely proportional to the particle mass, and is of the order of one Å. Hence ξ for a pair of deuterons would be of the order of 1/3500 of the electronic ξ or a few Fermis in diameter. This length is in the range of nuclear forces of the deuteron. This could easily lead to fusion and the formation of ^4He.

The work of Alfred Coehn cited by Pons and Fleischmann demonstrates that a positive charge is really the mobile carrier in PdD_x. The deuteron or a strongly coupled pair of deuterons are very reasonable choices for such a carrier, just as the strongly coupled electron-pair is known to be itinerant in the case of superconductivity.

The example of efficient proton (deuteron) carriers like the perovskites, which are also thought to be prime candidates for the formation of bipolarons, could lead to pair formation of two deuterons. The field of high temperature candidates for cold fusion, such as the work of Biberian or of the Russians needs the same careful experimental confirmation which has gone on in the palladium plus deuterium experiments.

Acknowledgments

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About the Author

Dr. Waber obtained a Bachelor of Science in chemical engineering in 1941 from the Illinois Institute of Technology, followed by a Masters degree in chemical engineering in 1943 from the same institution. He received his Ph.D. in metallurgy in 1946 from the Illinois Institute of Technology and specialized in the study of the stress corrosion cracking of mild steel. He was employed by Los Alamos Laboratory from 1947 until 1966. He has been a member of the academic staff of a number of universities and is currently Professor of Physics at Michigan Technological University. Waber has received various national awards and is author of over 150 papers. He has edited four conference volumes and received three U.S. patents.

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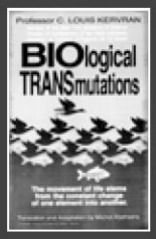

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Device continued from page 21

has been described in some articles about this technology in the magazine.[1,2] Eugene Mallove initially visited Stringham in his California laboratory to observe his methods of power measurement, data collection, and analysis. Calorimetric measurement of the thermal losses from the external oscillator circuit for the ultrasonic transducers determines the input electrical power to the reactor transducers. Calibration of the reactor proper with an internal joule heater allows determination of output power during experiments.

The overall conclusion of the site visit was that measurements were being properly made and analyzed, such that at least a two-fold amplification of input electrical power to the transducers appears to be achieved by Stringham, *e.g.* 10 electrical watts input, 20 watts thermal output. However, the excess heat power "rides" on top of an additional 50 watt input from joule heating, required in the present configuration to raise the temperature of the argon gas-pressurized heavy water high enough for the cold fusion effects to appear. In principle, this drawback could be eliminated by insulating the reactor. We will evaluate Stringham's reactor at NERL, first confirming its performance to our satisfaction using his protocols and then working with First Gate Energies to create a user-friendly demonstration device.

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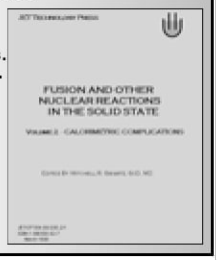

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21. Tittel, W. *et al.*, "Violation of Bell Inequalities. . ." He, incidentally, gives a figure of 0.707 instead of 0.5, for the test limit, but this is presumably because he looks at a different curve.

22. A coincidence curve is a plot of coincidence rate of a selected type, usually "++," against the setting of one of the detectors, the other being held fixed. (It is, incidentally, assumed that the curve is independent of the value chosen for the fixed detector—a point that is not always carefully checked.) The question is, is its visibility of any significance?

23. It is not easy to test what curve probabilities really obey, as all sources of light produce signals of varying strength. Experimenters in practice make adjustments until the system appears to obey Malus' Law, but they have no guarantee that they are not seeing the result of averaging over different shapes of curve, with the cancellation of any discrepancies from pure sinusoidal shape. There are circumstances in which correct behavior of the average is not enough.

24. Marshall and Santos recognized this problem back in 1985 and challenged Aspect, unsuccessfully. Paul Wesley discovered it independently and has published a paper on it (Reference 5 above). Aspect relied on many QM arguments to defend his case.

25. Aspect, A. 1983. "Trois Tests Expérimentaux des Inégalités de Bell par Mesure de Correlation de Photons," Ph.D. thesis No. 2764, Université de Paris-Sud, Centre D'Orsay.

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27. John Bell made a famous remark about not understanding how an experiment that worked so well with imperfect detectors could fail with perfect ones. Aspect convinced himself that the likely error from the detection loophole was negligible in his experiment, and that subtraction of accidentals was legitimate. I have reason to believe that his error estimate was erroneous, and his arguments regarding accidentals based on quantum theory assumptions that have not been proved.

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About the Author

Caroline Thompson received a Bachelor's degree in mathematics from Cambridge University in 1965 and a Master of Science in biometry from Reading University in 1969. A few years as a statistician were followed by non-academic pursuits until she restarted her education at Aberystwyth, where she earned a Master of Science in computer science in 1992. In 1993, Thompson came across Aspect's famous EPR experiments, found "realist" explanations, and began publicizing her findings.

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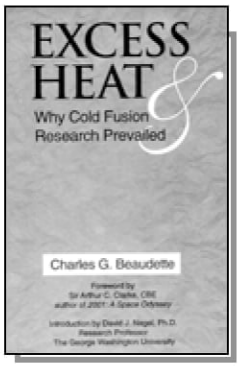

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